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# INDUSTRIAL CONFLICT AND BUSINESS FLUCTUATIONS[1]

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## I

It has long been recognized that there are cycles of strikes and that they are probably related to the business cycle. If the forces which tend to create a pattern of regularity in strike fluctuations can be identified, it is important to do so. Unless we learn to understand this pattern, cyclical changes in the level of strikes may be mistaken for permanent changes in the economic, social, or psychological determinants of industrial conflict. Since strikes are often the subject of legislation or other government action, mistakes in the diagnosis of strike fluctuations may contribute to errors in public policy.

Without looking at the data, one might expect either a positive or an inverse correlation between strikes and the business cycle. An expectation of a positive correlation would follow from the view that strikes are typically conducted by "business unions," which are always seeking gains for their members, and will adjust their strategy to business fluctuations so as to maximize these gains. The expectation of an inverse correlation would result from the view that strikes are essentially a protest of workers against intolerable conditions and that the conditions which provoke strikes are most common during depressions.

It should be possible to choose between these views on the basis of empirical evidence. Unfortunately, various students of the question have not agreed on the nature of the relation between strikes and business fluctuations. Hansen, analyzing strike data from 1881 to 1919, concluded that "strikes correlate inversely with the business cycle in periods of long run falling prices, while they correspond directly with the business cycle in periods of long run rising prices."[2] Griffen, analyzing strike data for 1881 to 1937, concluded that the troughs of his strike cycles corresponded generally with the years of depressions, and ten of the

[1] I am indebted to Professor Milton Friedman for valuable advice especially in connection with the statistical methods used. Professors Joel Seidman and Harold L. Wilensky have also made helpful suggestions. Albert Guttenberg, George S. Colburn, and Carla Zingarelli did the computation.

[2] Alvin H. Hansen, "Cycles of Strikes," *American Economic Review*, XI (1921), 620.

fifteen peaks of strike cycles corresponded with what he considered to be years of either recession or depression.[3] Yoder, analyzing the same period as Griffen, concluded that sharp increases in strikes were associated with increases in business activity, while sharp decreases in strikes were associated with decreases in business activity. However, he cautions us that "no simple covariation such as might be measured by linear correlation is observable. For the forty-nine years for which numbers of strikes are available, the linear measure of correlation between annual measures of strikes and indexes of business activity is $r = 0.11$ (1 per cent limit: 0.36)."[4]

The principal weakness of the studies mentioned so far lies in their reliance in whole or in large part on annual data. As Burns and Mitchell conclude after much study, "annual data are exceedingly crude materials for comparing the cyclic behavior of different activities in the same period or of the same activity in different periods. They obscure timing relations, they make it impossible to trace cyclical patterns with confidence, often they obscure and sometimes they obliterate cyclical fluctuations."[5]

In a recent study, Jurkat and Jurkat use monthly data, analyzing them by the methods developed by the National Bureau of Economic Research.[6] The Jurkats' work shows a high positive conformity between strikes and the business cycle. This study follows the Jurkats in applying the National Bureau method to monthly data. However, it carries the analysis further than the Jurkats did, making possible greater precision in measuring the cyclical patterns.[7] The need for further work was indicated by the conflicting and somewhat inconclusive nature of the findings of previous studies.

Use of the National Bureau method makes possible comparison of the cyclical behavior of strikes and many other activities which the National Bureau has analyzed. A brief account of the method will be given in the following section.

## II

The Bureau of Labor Statistics series on strikes beginning in each month has been used as the basic measure of strikes.[8] This series was chosen primarily because it is the only monthly series that goes back as far as 1915. It also has the important additional advantage that each month's observation is independent of the observations for other months, thus making the series more sensitive to changes in the conditions causing strikes. In contrast, the number of man-days idle in a given month includes the days lost from strikes starting in prior months.

[3] John I. Griffen, *Strikes: A Study in Quantitative Economics* (New York: Columbia University Press, 1939), p. 49. See n. 12 below.

[4] Dale Yoder, "Economic Changes and Industrial Unrest in the United States," *Journal of Political Economy*, XLVIII (1940), 222–37.

[5] Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles* (New York: National Bureau of Economic Research, 1946), p. 43.

[6] Ernest H. Jurkat and Dorothy B. Jurkat, "Economic Function of Strikes," *Industrial and Labor Relations Review*, II (July, 1949), 527–45.

[7] In the analysis which follows, the only departure from the National Bureau method of which I am aware arises because I have not, as the Bureau would have, used the combined judgment of several observers working independently in making some of the subjective decisions involved. These concern especially the adequacy of the seasonal correction and the dating of the turning points in the strike cycles.

[8] Florence Peterson, *Strikes in the United States, 1880–1936* (U.S. Bureau of Labor Statistics Bull. 651 [Washington: Government Printing Office, 1937]). For a discussion of the methods of collection of these data and their consistency through the period see *ibid.*, pp. 35–36. Data after 1936 are from the *Monthly Labor Review*. The data include both strikes and lockouts.

The measure of the business cycle that I have used is the reference cycle of the National Bureau of Economic Research. The reference cycle is based on the analysis of a large number of series on a wide variety of economic activities. It dates the expansions and contractions of aggregate economic activity but does not attempt to assign any amplitude to them. This measure of the business cycle is widely recognized as being by far the best available.[9]

The monthly data published by the Bureau of Labor Statistics show pronounced seasonal fluctuation and were therefore deseasonalized by use of the method of ratios to moving averages.[10] The deseasonalized data were then broken into strike cycles. Cycles in any particular series are called "specific cycles" to distinguish them from the cycles of general economic activity, or "reference cycles." In accordance with the National Bureau's method, a wave-like movement in the deseasonalized strike series was recognized as a specific cycle if it lasted at least fifteen months whether measured from peak to peak or from trough to trough. Short movements were ignored as erratic.[11] To help identify strike cycles after 1927, a second series was used: the number of workers involved in strikes beginning in the month. This series is so erratic that it had to be smoothed by a five-month moving average to reveal its cyclical movements. It was therefore used only as a rough check on the first series.

The analysis revealed a high correspondence between strike cycles and the business cycle for the period 1915–38 and very little correspondence for the war and postwar years, 1939–50. It will therefore be convenient to treat these two periods separately during much of the following discussion. The lack of correspondence during the second period occurs for three reasons: (1) There was a sharp drop in strikes from September, 1941, to November, 1942, associated with America's entry into the war and not corresponding to any contraction in business activity. (2) The mild contraction in business activity from February to October, 1945, was accompanied by an increase in strikes. The effect of the contraction on strikes was apparently outweighed by the removal of restraints

[9] The Bureau has not published its reference dates after 1938. Its tentative reference dates for the years 1939–49 were supplied by Milton Friedman.

[10] For the period 1927–36 Yoder's seasonal index was used (Dale Yoder, "Seasonality in Strikes," *Journal of the American Statistical Association*, Vol. XXXIII [December, 1938]).

For the periods 1915–26 and 1937–50 the use of this index did not completely eliminate seasonal fluctuation, and two new indexes were computed by similar methods. The amount of seasonal fluctuation as measured by these three indexes was greatest in the period 1915–26 and least in the period 1937–50. This raises the question: Why should the degree of seasonality decrease over time?

An important strike activity affected by seasons is picketing, especially mass picketing. The smaller seasonal fluctuation in strikes in the later periods thus may reflect increased union strength and the decreased dependence of unions on picketing to make strikes successful. In part the seasonality of strikes is a reflection of the seasonality of production in certain unionized industries, with strikes tending to occur at seasonal peaks when employers are most vulnerable. A reduction of the seasonality of production in these industries, or some changes in the distribution of strikes by industry, would reduce the seasonality of strikes. Other factors that may contribute to decreased seasonality are discussed by Mr. William Goldner, of the University of California, in his unpublished paper, "Strike Waves as a Symptom of Economic and Political Readjustment." I am indebted to Mr. Goldner for allowing me to see this paper.

[11] For a complete description of the methods of identifying specific cycles, as well as other aspects of the National Bureau's method, see Burns and Mitchell, *op. cit.* The terms "specific cycle" and "reference cycle" are those of the National Bureau. I shall also speak of "reference cycle patterns of strikes"—the movement of strikes during the periods defined by the cycle of general economic activity.

on striking at the end of the war. (3) There was a sharp drop in strikes from April to November, 1947, associated with the passage of the Taft-Hartley Act and not corresponding to any contraction in business activity.

For the period 1915–38 it was found that there was a cycle of strikes corresponding to each reference cycle. In addition, there was one "extra" strike cycle which, measured from trough to trough, lasted from October, 1930, to October, 1932. An extra specific cycle during this general period is not peculiar to the strike series; it is found also in some other unrelated series. Where the strike cycles correspond to the reference cycles, the turning points can be computed to show lags and leads. This comparison is made in Table 1.

The turning points of the specific cycles shown in Table 1 were determined from the deseasonalized strike data; the peaks and troughs of the reference cycles are those determined by the National Bureau. The lack of timing comparison for the strike trough of October, 1930, and the strike peak of January, 1930, is due to the "extra" strike cycle discussed above. No timing comparisons are possible after 1940 because of the lack of correspondence between the specific and reference cycles.

Table 1 shows a fairly clear tendency for strikes to lead at the upper turning point[12] and to lag at the lower turning point. In no case do strikes lag at the peak, and only in 1932 do they lead at the trough. The time between the reference turn and the strike turn averages four months at the peaks and six months at the troughs.

TABLE 1

TIMING OF SPECIFIC CYCLES, STRIKES BEGINNING IN EACH MONTH, 1915–40

| Dates of Specific Cycles | | | Timing at Reference Peak | | Timing at Reference Trough | |
|---|---|---|---|---|---|---|
| Trough | Peak | Trough | No. of Months Lead (−) or Lag (+) | Date of Reference Peak | No. of Months Lead (−) or Lag (+) | Date of Reference Trough |
| | Dec. 17 | Apr. 19 | −8 | 8/18 | 0 | 4/19 |
| Apr. 19 | Sept. 19 | June 22 | −4 | 1/20 | +9 | 9/21 |
| June 22 | May 23 | Aug. 24 | 0 | 5/23 | +1 | 7/24 |
| Aug. 24 | Aug. 25 | June 28 | −14 | 10/26 | +6 | 12/27 |
| June 28 | Apr. 29 | Oct. 30 | −2 | 6/29 | * | * |
| Oct. 30 | Jan. 32 | Oct. 32 | * | * | −5 | 3/33 |
| Oct. 32 | Mar. 37 | June 40 | −2 | 5/37 | +25 | 5/38 |
| Average* | | | −5 | | +6 | |
| Average deviation | | | 4.0 | | 7.3 | |

* No timing comparison made.

For further study the specific cycles are broken into nine stages. Stage I is the three months centered on the initial trough; Stages II through IV are succes-

[12] This finding illustrates the dangers of using annual data. Griffen (*op. cit.*, p. 49) recognizes strike peaks in 1923, 1929, and 1937 and in each case identifies them with a recession in general business activity. The monthly data show a correspondence with the peak in 1923 and a two-month lead in the other two cases.

sive thirds of the expansion; Stage V is the three months centered on the peak; Stages VI through VIII are successive thirds of the contraction; and Stage IX is the three months centered on the terminal trough. For each stage the average number of strikes per month is expressed as a ratio to the average number of strikes per month in the full specific cycle. These ratios are called "specific cycle relatives." The use of cycle relatives as a unit of measurement permits comparison of strike cycles with cycles of activities normally measured in diverse kinds of units.[13]

The specific cycle pattern of strikes is shown in Table 2. Each horizontal line represents a specific cycle. Data for months before April, 1919, and after November, 1947, are omitted because they do not form complete cycles. It can be seen that, even in this condensed form, the strike cycles are not perfectly regular. Five of the eight cycles do not rise and fall smoothly from stage to stage, and several have one trough much lower than the other. However, the average pattern of the eight cycles is smooth and symmetrical.

The difference between Stage I and Stage V of each cycle is the amplitude of the rise in specific cycle relatives; that between Stage V and Stage IX, the amplitude of the fall. The sum of these differences is the amplitude of rise and fall. The average amplitude of the rises is

TABLE 2

SPECIFIC CYCLE PATTERNS OF STRIKES BEGINNING IN EACH MONTH, APRIL, 1919—NOVEMBER, 1947

| Dates of Specific Cycles | | | Average in Specific Cycle Relatives at Stage | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Trough | Peak | Trough | I | II | III | IV | V | VI | VII | VIII | IX |
| | | | Trough | Expansion | | | Peak | Contraction | | | Trough |
| Apr. 19 | Sep. 19 | June 22 | 94.0 | 117.1 | 140.7 | 167.0 | 182.0 | 127.4 | 98.7 | 52.1 | 31.4 |
| June 22 | May 23 | Aug. 24 | 68.9 | 85.4 | 83.0 | 115.8 | 135.2 | 114.7 | 103.6 | 93.0 | 73.8 |
| Aug. 24 | Aug. 25 | June 28 | 99.2 | 111.0 | 131.8 | 129.9 | 135.8 | 113.3 | 93.9 | 67.5 | 63.1 |
| June 28 | Apr. 29 | Oct. 30 | 77.8 | 73.5 | 88.5 | 102.4 | 140.0 | 123.3 | 103.3 | 87.4 | 84.2 |
| Oct. 30 | Jan. 32 | Oct. 32 | 76.8 | 85.7 | 93.9 | 112.7 | 128.1 | 105.6 | 94.6 | 105.6 | 93.4 |
| Oct. 32 | Mar. 37 | June 40 | 30.9 | 62.8 | 79.8 | 94.2 | 218.6 | 160.8 | 114.7 | 97.2 | 92.8 |
| June 40 | Sep 41 | Nov 42 | 66.6 | 80.2 | 110.3 | 122.9 | 146.9 | 106.2 | 85.6 | 92.7 | 76.7 |
| Nov 42 | Oct. 46 | Nov. 47 | 59.4 | 90.5 | 106.8 | 111.9 | 126.3 | 101.4 | 99.8 | 65.1 | 59.3 |
| Average | | | 71.7 | 88.3 | 104.4 | 119.6 | 151.6 | 119.1 | 99.3 | 82.6 | 71.8 |
| Average deviation | | | 15.3 | 13.5 | 18.0 | 15.3 | 24.3 | 13.6 | 6.1 | 15.8 | 15.4 |
| | | | Months | | | | | | | | |
| Average interval between stages* | | | .... | 3.8 | 6.7 | 6.7 | 3.8 | 4.1 | 6.9 | 6.9 | 4.1 |

* Measured from the midpoint of the preceding stage to the midpoint of the stage in which the figure is entered. The total of this line, 42.9 months, is the average duration of the specific cycles. (Detail does not add to total because of rounding.)

[13] Stage IX of a given cycle covers the same months as Stage I of the succeeding cycle but is expressed on a different base. This helps to show the way in which the method used abstracts from the intercycle portion of secular trends—that is, it ignores changes in the general level of strikes from one cycle to the next.

79.9, that of the falls is 79.8, and the average amplitude of the rise and fall is 159.7.

The proceeding discussion gives a picture of the strike cycle; other measures are needed to see how well it conforms to the business cycle. For these, the data are broken into reference cycles, and reference cycle relatives are computed. The reference cycle relatives and the stages of the reference cycle are exactly analogous to the specific cycle relatives and stages discussed above.

The reference cycle patterns of strikes are shown in Table 3. To isolate the effect of the lack of conformity in the war and postwar years, averages are shown separately for the first five reference cycles and for all seven. As would be expected, the reference cycle patterns are less regular than the specific cycle patterns. The highest level of strikes occurs in the peak stage of the reference cycle only twice in eight cycles. However, the averages of both the first five and all seven full reference cycles show a regular and almost symmetrical rise and fall, with the peak in Stage V. The average amplitude of the reference cycles is less than that of the specific cycles, since the differences in timing between strike cycles and the reference cycle have a

TABLE 3*

REFERENCE CYCLE PATTERNS OF STRIKES BEGINNING IN EACH MONTH, JANUARY, 1915—OCTOBER, 1949

| DATES OF REFERENCE CYCLES | | | AVERAGE IN REFERENCE CYCLE RELATIVES AT STAGE | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | I | II | III | IV | V | VI | VII | VIII | IX |
| Trough | Peak | Trough | Trough | Expansion | | | Peak | Contraction | | | Trough |
| Dec. 14 | Aug. 18 | Apr. 19 | | 54.1† | 119.3† | 123.0† | 95.6† | 78.8† | 137.8† | 88.0† | 83.6† |
| Apr. 19 | Jan. 20 | Sep. 21 | 81.5 | 115.2 | 158.0 | 121.1 | 112.5 | 106.6 | 87.1 | 74.2 | 47.4 |
| Sep. 21 | May 23 | July 24 | 117.2 | 118.7 | 73.2 | 98.2 | 132.1 | 106.0 | 108.3 | 94.8 | 77.1 |
| July 24 | Oct. 26 | Dec. 27 | 101.5 | 114.4 | 123.7 | 102.9 | 97.8 | 79.9 | 83.1 | 59.6 | 64.1 |
| Dec. 27 | June 29 | Mar. 33 | 79.2 | 77.9 | 78.4 | 109.6 | 116.7 | 98.0 | 97.3 | 113.9 | 139.9 |
| Mar. 33 | May 37 | May 38 | 42.8 | 70.4 | 81.2 | 111.9 | 225.0 | 186.2 | 129.1 | 115.7 | 104.4 |
| May 38 | Feb. 45 | Oct. 45 | 73.6 | 71.1 | 99.1 | 119.0 | 115.2 | 127.2 | 131.6 | 153.3 | 160.8 |
| Oct. 45 | Nov. 48 | Oct. 49 | 146.5 | 123.7 | 99.6 | 83.7 | 79.7 | 92.4 | 97.2 | 87.1 | 77.4 |
| Average, first five full cycles‡ | | | 84.4 | 99.3 | 102.9 | 108.7 | 136.8 | 115.3 | 101.0 | 91.6 | 86.6 |
| Average deviation | | | 19.9 | 20.1 | 30.4 | 6.6 | 35.3 | 28.3 | 14.2 | 19.8 | 28.5 |
| Average, seven full cycles | | | 91.8 | 98.8 | 101.9 | 106.6 | 125.6 | 113.8 | 104.8 | 99.8 | 95.9 |
| Average deviation | | | 25.7 | 22.0 | 22.3 | 10.0 | 30.3 | 24.6 | 15.6 | 23.9 | 33.6 |
| | | | Months | | | | | | | | |
| Average interval between stages, first five full cycles‡ | | | | 4.5 | 7.9 | 7.9 | 4.5 | 3.8 | 6.7 | 6.7 | 3.8 |
| Average interval between stages, seven full cycles§ | | | | 6.1 | 11.1 | 11.1 | 6.1 | 3.2 | 5.6 | 5.6 | 3.2 |

* A table which is exactly comparable in conception to this one appears in Jurkat and Jurkat, *op. cit.*, p. 534. It differs substantially in detail, however, apparently because of differences in the method of deseasonalizing the original data.

† Based on the incomplete cycle starting January, 1915. Excluded from the averages and average deviations.

‡ The total of this line, 45.8 months, is the average duration of the first five full reference cycles.

§ The total of this line, 52.3 months is the average duration of the seven full reference cycle. (Detail does not add to total because of rounding.)

flattening effect. The average amplitude of the rise in reference cycle relatives for the first five full reference cycles is 52.4, of the fall 50.2, and of the rise and fall 102.6. These are substantial amplitudes and in themselves are an indication that strikes conformed to the business cycle during the period from 1919 to 1938. They are considerably higher than the reference cycle amplitudes of the eighth decile, when arrayed by ascending reference cycle amplitudes, of 794 series analyzed by the National Bureau.[14] When compared to the behavior of 574 series grouped by twenty-nine subjects, this strike series during the interwar period has a higher reference cycle amplitude for the full cycle than the average of any groups except corporate security issues, new orders from manufacturers, and private construction contracts. It ranks higher than any group of series on production, prices, inventories, employment, or payrolls.[15]

For the seven full reference cycles from 1919 to 1949, the average amplitude of the rises is 33.8, of the falls 29.7, and of the rises and falls 63.5. These amplitudes are higher than the reference cycle amplitude of the seventh decile, when arrayed by ascending reference cycle amplitude of the series analyzed by the National Bureau. Compared with the average amplitudes of groups of series by subject, the full-cycle amplitude for this period is exceeded by the three groups mentioned above, plus production of durables; payrolls of durable goods industries; and inventories, inverted timing.[16] It is higher than the average of any group of series on employment or prices.

Indexes of conformity to the business cycle are constructed from the reference cycle relatives at Stages I, V, and IX. The index of conformity to the reference expansion is arrived at by entering +100 whenever the series rises during a reference expansion and −100 whenever it falls during a reference expansion and averaging these entries. Since the strike series rises during four of the first five reference expansions, its index of conformity to expansions is +60 for the period 1918–38.[17] The exception occurs in the expansion of July, 1924—October, 1926, and results from the long lead in the strike peak. The index of conformity to the reference contraction is exactly analogous, with the directions reversed. Since the series falls during five of the first six reference contractions, the index of conformity to contractions is −67 for the period. The exception occurs in the contraction from June, 1929, to March, 1933, and results from the lead in the strike series at the trough.

The index of conformity to the full cycle is based on the rates of change per month during successive phases of the cycle. Even if a series rises during both phases of a reference cycle, it is considered to conform to the full cycle, trough to trough, if the rate of rise is less during the reference contraction than

[14] Wesley C. Mitchell, *What Happens during Business Cycles* (New York: National Bureau of Economic Research, 1951), p. 102.

[15] *Ibid.*, p. 106. It should be noted, however, that none of the twenty-nine groups contains fewer than six series and that many of the series extend over longer periods of time. The group averages are hence more reliable than the single strike series. A group of series on the number of strikes might show a higher or lower amplitude than the single series used. There are undoubtedly individual series in the groups ranking below the strike series which have higher amplitude than the strike series.

[16] That is, those which increase during business contractions and decrease during expansions.

[17] The reference expansion of December, 1914—August, 1918, is omitted, since data are not available for its first month. The contraction of the same cycle is included, so that the index of conformity to contractions includes one more cycle than the index of conformity to expansions.

during the preceding reference expansion. Similarly, it is considered to conform to the full cycle, peak to peak, if the rate of rise during a reference contraction is less than that during the following reference expansion when both are measured on the same base. For the period between 1918 and 1939 the strike series has an index of conformity to the full cycle of +100 both from peak to peak and from trough to trough.

The index of conformity to expansions of the strike series for the interwar period is the same as the index of conformity of the fourth decile, when arrayed by ascending indexes of conformity, of the 794 series analyzed by the National Bureau.[18] For the reference contractions it is above that of the median of the 794 series, and for the full cycle it is the same as that of the seventh decile. When the National Bureau series are classified into five broad areas of subject matter, we find the strike series conforming less well than the averages of all series in each of these areas: construction contracts; production; payrolls, and other income payments; and employment. However, it conforms better than the average of 147 series on prices. It also conforms better than any subgroup of prices, except prices of durable goods, which conform better to the contraction.[19] This is noteworthy because price movements are generally considered an essential part of the business cycle, while strikes generally are not.

When the war and postwar period is included, the indexes of conformity drop. For the period 1918–49 the index of conformity to expansions is 43, to contractions 50, and to full cycles 57. The principal reason for the drop in the conformity indexes is the rise in strikes during the mild contraction of business activity between February and October, 1945. If this mild contraction had not been recognized by the National Bureau, we would have one long reference cycle with an initial trough in May, 1939, a peak in November, 1948, and a terminal trough in November, 1949. The behavior of strikes would conform during the war and postwar period to reference cycles defined in this way. The reference cycle patterns, reference cycle amplitudes, and indexes of conformity reinforce one another in demonstrating a significant relationship between the number of strikes and the business cycle—a much more significant relationship than that suggested by most of the previous studies. Before examining the possible reasons for this behavior, let us consider briefly the cyclical patterns of strike issues and outcomes.

## III

The Bureau of Labor Statistics has published data continuously since 1915 on the major issues involved in strikes, and from 1915 to 1943 it published data on the results of strikes. Data prior to 1927 are not entirely comparable with the later data. Unfortunately, these data are available only on an annual basis and hence are very crude for the purposes of measuring cyclical fluctuations. In most of the series the minor cycles are obscured entirely.

The issues involved are classified into three major groups: wages and hours, organization, and miscellaneous. Strikes involving both organization and wage issues are included under organization, as are strikes over union-security issues. The percentage of strikes in the category

18 Mitchell, *op. cit.*, p. 81.

19 *Ibid.*, p. 92. The caution voiced in n. 15 applies even more strongly here, since the indexes of conformity are so insensitive. For five cycles no values between +60 and +100 are possible. For further discussion see *ibid.*, pp. 83–90.

"wage and hours" rises during the depression of 1921 and rises sharply after 1929. Wage-and-hour strikes were 40.4 per cent of all strikes in 1929 and 65.7 per cent in 1932. However, the proportion of strikes involving wages fell slightly in the "recessions" of 1938 and 1949. The proportion of strikes involving organization falls in 1921, from 1929 to 1932, in 1938, and again in 1949. Strikes involving union recognition follow a pattern similar to that of all strikes involving "organization."

Strikes involving wage decreases can be separated from all wage-and-hour strikes. As a fraction of all wage-and-hour strikes, strikes involving wage decreases show an extraordinary sensitivity to the business cycle. Strikes involving wage decreases rise from 7.2 per cent of wage-and-hour strikes in 1920 to 64.8 per cent in 1921. They fall sharply in the cyclical peak of 1923, rise in the trough of 1924, and fall again in the peak of 1926. From 34.6 per cent of all wage-and-hour strikes in 1929, those involving wage decreases rise to 77.0 per cent in 1932. They fall to 4.7 per cent in the revival of 1937 and rise again to 35.2 per cent in 1938. They rise slightly in the trough of 1949, but throughout the period after 1940 they are at a very low level. In only one case does the series fail to correspond to a cyclical peak or trough: it does not fall in the peak of 1919. In 1935 it has a pronounced rise not associated with a cyclical trough; this was probably a result of the invalidation of the National Industrial Recovery Act.

The results of strikes are classified into the categories substantially won, compromised, lost, and others. The "substantially won" category and the "lost" category respond to the major troughs of the business cycle. Strikes won fall sharply in 1921 and fall in 1930 and 1938. Somewhat surprisingly they rise from 1930 to 1932. Strikes lost rise sharply in 1921, rise from 1929 to 1932, and rise somewhat in 1938.

Data on the average duration of strikes are available annually since 1927 but show no relation to the business cycle.

## IV

What is the meaning of the positive correlation of strikes and the business cycle? One approach suggested by the lead of strikes at the peaks would be to regard the relation as one in which strikes are a casual force and changes in business conditions, at least in part, a result of strike fluctuations. Griffen advances such a theory, although with some hesitancy. He argues that a strike peak, by adversely affecting the expectations of businessmen, would cause a downturn in economic activity.[20] The Jurkats also argue that strikes are a determinant of general business conditions but in a different way. They hold that strikes, by increasing wages, provide purchasing power and thus help expand production during the upswing.[21] Both views seem to overestimate the importance of strikes, and the second has the additional fault of overlooking the depressing effects of strikes and wage increases through increases in costs and the generation of pessimistic expectations.

The Bureau of Labor Statistics has estimated the time lost in strikes as a percentage of total time worked for each year since 1927. The highest prewar figure was 0.43 per cent in 1937; the lowest was 0.05 per cent in 1930. The

[20] *Op. cit.*, pp. 51 and 204. Griffin ascribes this view to A. C. Pigou. However, he clearly seems to have misinterpreted him. See A. C. Pigou, *Industrial Fluctuations* (London: Macmillan & Co., 1927), pp. 45-47.

[21] *Op. cit.*, p. 540.

all-time high of 1.43 per cent occurred in 1946. These figures may somewhat understate the real loss, since strikes also cause idleness for many nonstrikers. On the other hand, some strike losses are recovered after the strike is over by increasing employment or hours to levels that they would not have reached if the strike had not occurred. The small magnitude of the strike loss indicates that we should abandon the notion that strikes are a cause of the business cycle.

The correlation can be much better explained in the opposite direction. Such an explanation does not need to assume that the timing of all strikes is determined by the business cycle. There will, of course, be many strikes whose timing is unrelated to business conditions. It is sufficient that the timing of a considerable proportion of strikes is determined by the cycle.

A cyclical fluctuation in the number of strikes could occur for any of three reasons: (1) The propensity to strike of workers in a given unit (department, plant, company) could vary with the cycle. (2) The number of such units organized by unions could fluctuate with the cycle; since the great majority of all strikes are conducted by unions,[22] this could cause the number of strikes to fluctuate even though the propensity to strike in each organized unit remained constant. (3) The scope of strikes could change during the cycle, so that during the expansion strikes took place in small units (departments, plants), while during the contraction they took place in large units (companies, industries). This would cause cyclical variation in the number of separate strikes reported without any variation in the propensity of given workers to strike.[23]

Rough statistical checks can be made on the last two possibilities. No data are available on the number of organized units, but the number of union members serves as a crude substitute. The fluctuation in actual union membership is probably somewhat greater than that of the reported membership, since unions are reluctant to admit membership losses. Union membership figures show declines in the depressions of 1921 and 1929–32 which may help to explain declines in the number of strikes. At other times, however, union membership moves counter to the strike series; this is especially true in the expansion of 1922–23 and the contraction of 1938. Even when the movements are in the same direction, the strike movements are much larger. Thus cyclical fluctuation in union membership is at best a minor contributing cause of the cyclical fluctuation in strikes.

Data on the number of strikes involving one, two to five, and more than five establishments are available annually since 1917. These series show no cyclical fluctuation. Cyclical variation in the scope of strikes does not seem to be a cause of the fluctuation in their number. Changes in the propensity to strike thus seem to be the principal cause of cyclical strike fluctuations. Why should these conform to the business cycle?

The principal economic factor affecting union behavior is the state of the labor market—the amount of employment available. Employment is very highly correlated with the business cycle.[24] Strikes occur during periods of rising employment to secure wage in-

[22] From 1927 to 1936 strikes which did not involve unions varied between 6 and 19 per cent of all strikes. In recent years the corresponding figure has been much lower; usually below 2 per cent.

[23] I am indebted to Professor H. Gregg Lewis for suggesting possibilities (2) and (3).

[24] See Mitchell, *op. cit.*, p. 92.

creases and other benefits in unionized plants and also to organize the unorganized.[25] Rising employment and improving business conditions offer the unions a variety of strategic advantages: the employer's reluctance to lose his share of the expanding market, and his observation of rising wages elsewhere lowers his resistance to union demands. His ability to replace strikers with nonstrikers diminishes as employment rises, and the strikers have an increased chance of obtaining employment elsewhere if the employer succeeds in replacing them. If the expansion produces a rising cost of living, workers in firms whose wages lag behind this rise will protest.

In periods of falling employment there is a sharp drop in organizational strikes. Employees in nonunion establishments become afraid to form unions for fear that they can easily be discharged and replaced by others. Employees already organized will also be afraid to strike when they see the bargaining power of the employer rise relative to theirs. The employer's hand may be strengthened by his ability to fill orders out of inventory or to make up lost production after the strike is over. The union may fear that a strike or the granting of economic concessions by the employer would damage his competitive position and thus imperil the jobs of some union members. This will be especially true where the employer faces competition from nonunionized employers. Moreover, workers who keep full-time jobs during the early stages of a depression usually get real wage increases without striking. These are produced, on the one hand, by the general tendency of wages to be sticky downward, reinforced by fixed-term union contracts, and, on the other hand, by the fall in consumer prices. As the downswing progresses, a rising proportion of the diminishing number of strikes represents protests against wage reductions made or proposed by employers.

While so far an attempt has been made to explain the general correlation between strikes and the cycle, the timing at the peaks and troughs has not been explained. Why does the strike peak consistently precede the reference peak? The strike peak probably represents a maximum in the divergence of expectations between employers and unions. Unions pay close attention to employment, which generally does not lead at the peak.[26] They are also influenced by previous wage increases received by other unions and by increases in the cost of living. The attention of employers is likely to be focused on some of the activities which do lead at the peak, and they will thus resist demands for which the unions are still willing to fight. Among the series which lead at the peak, those which may be relevant are four series on the number of business failures or liabilities of business failures (inversely correlated so that they rise at these turns) and many series on investment, security issues, contracts, and orders.[27] Most of these turns reflect changed expectations on the part of businessmen. As the cyclical peak is reached, the more pessimistic expectations may be shared by some union leaders, and strikes fall off.

The prevailing lag of the strike series

[25] There are still a substantial number of strikes for union recognition despite the Wagner and Taft-Hartley acts. Some of these are in firms in intrastate commerce in states without comparable state laws; others represent a desire to avoid the delays and complications of representation elections.

[26] See Daniel Creamer, *Behavior of Wage Rates during Business Cycles* ("Occasional Paper," No. 34 [New York: National Bureau of Economic Research, 1950]), Charts 1 and 4.

[27] Mitchell, *op. cit.*, p. 71.

at the troughs is more simply explained. It seems to represent a "wait and see" attitude on the part of union leaders, who want assurance that the revival is genuine before risking the jobs of their members. The exceptional upturn of strikes in November, 1932, seems to be related to the election of President Roosevelt, which raised the hopes of unionists before his policies began to affect business conditions.

Political events, government policies, and the climate of public opinion all have an important influence on the timing of strikes and account for many deviations from the normal cyclical pattern.

The sharp impact of Pearl Harbor and the Taft-Hartley Act on the number of strikes is evidence of this. It could be argued that the conformity of strikes to the business cycle was a phenomenon of the interwar period and that primarily political determination of the timing of strikes, like that of the last ten years, can be expected to continue. However, disturbances in labor relations of the magnitude of Pearl Harbor and Taft-Hartley are rare, and it seems more likely that the timing of strikes will continue to respond to the business cycle unless the amplitude of changes in business activity is much smaller in the future than it has been in the past.

Strikes may follow a pattern of response to recurrent political events, such as elections, superimposed on the pattern of response to business cycles. But it is outside the scope of this paper to investigate this possibility.

If the business cycle is not the sole determinant of the timing of strikes, still less is it their cause. Most industrial disputes are caused by social and psychological forces or by economic forces which are noncyclical in nature. Many result from problems peculiar to labor-management relations in particular plants and firms. However, the cyclical pattern of strikes resembles the pattern of production of durable rather than nondurable consumers goods. This suggests that many of the grievances of industrial workers are "durable" or at least "semidurable." Grievances can be stored up for long periods. They are most likely to boil over into strikes, or to be utilized by union leaders as strike issues, when business conditions promise that the strikes may be successful. The same provocation which causes a strike when employment opportunities are rising might cause only grumbling during a depression.

It follows that the elimination of the business cycle would not necessarily reduce the number of strikes but might merely space them more evenly over time or alter the issues which occasion them. Changes in the cost of living would become less important as a cause of disputes, whereas the issue of wage increases to keep pace with rising productivity, now often adjusted as part of adjustments to cyclical changes, would become more important in its own right. However, some strikes seem to serve useful social and psychological functions, and therefore the elimination of all strikes is not necessarily a desirable objective. The stabilization of the general level of prices and greater stability in employment are desperately needed for many other reasons. They will continue to be sought eagerly, despite their failure to promise the elimination of industrial conflict.

# INFLATION AND SAVING BY CONSUMERS

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## INTRODUCTION

THE statistical series which estimate total savings by individuals have been showing annual saving of several billion dollars in the past few years. For 1949, for example, the Department of Commerce estimates personal saving at $6.3 billion. Since savings are "income not consumed," one might conclude that the total effective demand of consumers was substantially less than their disposable income and that the actions of consumers exerted pressure in a deflationary direction.

Such a conclusion would be incorrect. It fails to take into account offsetting decisions to invest, made by the same individuals. It is the purpose of this paper to introduce a concept—*deflationary saving*—designed to measure directly for each household the net effect of that household's decisions on the level of effective demand. By the deflationary saving definition two-fifths of all consumer units were negative savers in 1949. We estimate that the negative savings of these units more than offset the savings of the 53 per cent of the units whose savings were positive. That is, consumers as a whole used their entire income and also incurred debt and reduced their liquid assets to pay for goods and services purchased during the year. Although annual "savings" by some definitions are highly concentrated, deflationary savings are not. In 1949 and 1950 the several income groups contributed to the negative savings of the population for the year roughly in proportion to their numbers.

## TWO DEFINITIONS OF SAVING BY CONSUMERS

The difference between the usual definitions of saving by consumers and that used here may be stated with the aid of diagrams. Figure 1*A* shows the familiar relations between consumption, investment, and income in a "Keynesian" model simplified for expository purposes to a two-dimensional graph. The diagram refers exclusively to the consumer sector. Thus $Y$ refers to disposable cash income of consumers, $C_1$ to outlays for consumption by consumers, and $C_1$ plus $I$ to outlays by consumers for consumption plus outlays for investment in consumer goods.

Corresponding to $C_1$ is the following type of definition of savings:

$$S_1 = Y - C_1 .$$

Figure 1*B* shows $S_1$ as a function of $Y$; it is derived from Figure 1*A* in the usual manner. We refer to $S_1$ as "net worth savings," since a consumer's savings by this definition are equal to the change in

[1] We are indebted to Dr. George Katona and our other colleagues at the Survey Research Center for support and criticism and to Professor Kenneth E. Boulding, in whose seminar most of these ideas were first introduced. Most of the data used in this article were taken from interviews in the Surveys of Consumer Finances. These surveys are conducted by the Survey Research Center for the Federal Reserve Board. The authors are indebted to the Board of Governors of the Federal Reserve System for their sponsorship of the surveys and to the members of the Board's Division of Research and Statistics, who have worked with the staff of the Survey Research Center in developing these surveys.

his net worth. In practice, measures of change in net worth may differ according to how the distinction between consumption and investment is made. Several measures of net worth savings are discussed in the statistical section of this paper.

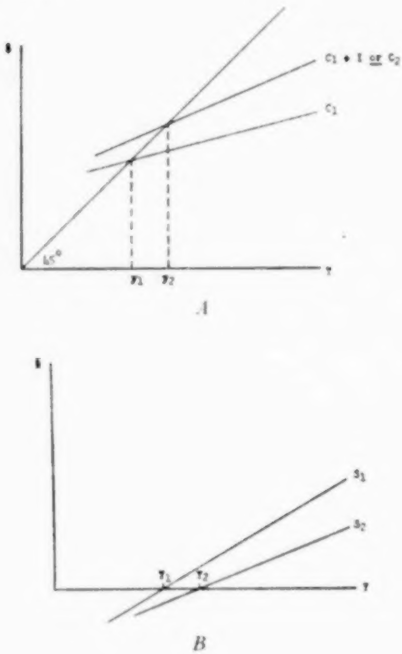

FIG. 1.—Relation of income, consumption, and saving: consumer sector only.

Deflationary savings by consumers may be defined as the difference between income ($Y$) and *total* outlay for goods and services ($C_2$):

$$S_2 = Y - C_2 \, .$$

If $S_2$ is positive, consumer decisions will have exerted a *deflationary* pressure. Figure 1*B* shows $S_2$ as a function of $Y$. Note that $S_1$ differs from $S_2$ by an amount equal to $I$. Measures of deflationary savings may vary according to what sectors of the economy are included, but there is not likely to be a conceptual variation in what constitutes cash outlay for goods and services. The magnitude of deflationary saving does *not* depend on the troublesome distinction between consumption and investment by consumers.

It is our contention that, for purposes of the statistical analysis of changes in the level of economic activity, the concept deflationary savings ($S_2$) has a great advantage over net worth savings ($S_1$). Whenever statistics based on a concept of net worth saving are used, the analysis must consider separately purchases by consumers of durables and real estate. These purchases are both saving and investment, but they are performed by the same people in the same actions. The interesting savings are those which were not automatically offset as they were made, and these savings are isolated in deflationary savings ($S_2$). Available statistics, where saving and investment data are presented in separate tables, do not emphasize the necessity of examining both net worth savings and investment. Deflationary savings ($S_2$) avoids this difficulty by presenting these facts as a single statistic.

Is $(C_1/Y)$ or $(C_2/Y)$ more appropriately labeled the "average propensity to consume"? For *purposes of analysis of changes in the level of economic activity* it seems to us that $(C_2/Y)$ is the proper ratio, since $C_2$ represents the total outlay for goods and services, including houses and durable goods, and it is this total outlay which subsequently will be received as income by providers of these goods and services.

Since $C_2$ contains elements of consumer investment which are known to vary over the cycle, we should expect $(C_2/Y)$ to be unstable through time. The definitive answer as to the stability of this ratio must await statistical measurement over an extended period.

A further theoretical implication is that the investment multiplier should be recast with a marginal propensity to consume, derived from $C_2$. If it should turn out that the relationship between $C_2$ and $Y$ is unstable through time, doubt would be cast on the usefulness of the investment multiplier as a predictive instrument.

Differences between a typical deflationary saving definition ($S_2$) and a typical net worth saving definition ($S_1$) may be illustrated by examining the accounts of a hypothetical consumer, $P_1$:

STATISTICAL ANALYSIS

The statistics presented here are for the consumer sector (with certain qualifications about the boundaries of the sector, discussed below). For the year 1949, measurements have been made for one variant of deflationary savings and for two variants of net worth savings by consumers, all based on data obtained in the 1950 Survey of Consumer Finances.[2]

The three definitions include, first, the definition used regularly in the Survey of Consumer Finances, referred to here as

[2] For a discussion of the Survey see "Methods of the Survey of Consumer Finances," *Federal Reserve Bulletin*, July, 1950.

| | Net Worth Savings ($S_1$) | | Deflationary Savings ($S_2$) | |
|---|---|---|---|---|
| | Plus | Minus | Plus | Minus |
| $P_1$ made a change in his housing arrangements during the accounting period. | | | | |
| (1) He sold a house for $20,000, paying off the $15,000 mortgage as part of the transaction | ...... $15,000 | $20,000 ...... | ...... $15,000 | ...... ...... |
| (2) $P_1$ bought a larger, better-located house for $30,000 and assumed a mortgage of $17,000 on it | 30,000 ...... | ...... 17,000 | ...... ...... | ...... $17,000 |
| (3) In tune with the prosperity of the time, he purchased a new car for $3,000, paying cash | 3,000 | ...... | ...... | ...... |
| (4) The rest of his $17,000 disposable income was used for the purchase of services and nondurable goods | | | | |
| | $48,000 | $37,000 | $15,000 | $17,000 |
| His net worth saving amounted to $11,000 | $11,000* | | | |
| while the total deflationary saving was—$2,000 | | | | −$ 2,000* |

His total spending for goods and services would be calculated as follows:

| | |
|---|---|
| Disposable income.......... | $ 17,000 |
| Gross receipts from sale of house... | 20,000 |
| Total inflow.......... | $ 37,000 |
| *Minus* deflationary savings.......... | − 2,000† |
| Total spending.......... | $ 39,000 |

The total spending of $39,000 breaks down into $30,000 for the house, $3,000 for the car, and $6,000 for services and nondurable goods.

* For either net worth or deflationary savings identical results may be obtained by summing the appropriate components of saving or by subtracting $C_1$ or $C_2$ from disposable income.

† This is an algebraic subtraction. Hence, change the negative sign and add.

"Net Worth I."[3] This definition includes house purchases as positive saving but excludes purchases of consumer durable goods. The second definition—"Net Worth II"—does include purchases of major durable goods as savings;[4] otherwise, it is identical with the first. No entries for depreciation are made under either definition.[5] The third definition—"Deflationary Savings"—excludes houses and consumers' durable goods.[6]

Table 1 shows frequency distributions for the three definitions. Both net worth definitions—but especially Net Worth II—show a larger proportion of positive savers and larger amounts of positive saving than Deflationary Savings, results which are not surprising, since both net worth definitions count more actions as savings than does Deflationary Savings.

Taken together, the negative figure for mean Deflationary Savings and the positive means for Net Worth I and II indicate that consumers as a whole drew down their liquid assets or borrowed money during 1949 but did so not to finance current consumption but to build up stocks of goods.

Table 2 shows in detail the difference between Deflationary Savings and Net Worth I Savings for identical spending units. A considerable portion of the negative savers under Deflationary Savings were positive savers under Net Worth I. Many of these spending units bought a house, or made large additions to a house, and borrowed to finance the transaction. The borrowing is entered as negative saving under the deflationary definition, and

TABLE 1*

COMPARISON OF FREQUENCY DISTRIBUTIONS FOR DIFFERENT DEFINITIONS OF SAVINGS IN 1949

| Amount Saved | Deflationary Savings | Net Worth I† | Net Worth II‡ (Including Durables) |
|---|---|---|---|
| Positive Savings | 52.6% | 59.7% | 74.0% |
| Plus $5,000 or more | 1.1% | 1.0% | 1.6% |
| Plus $2,000–$4,999 | 2.7 | 3.6 | 6.0 |
| Plus $1,000–$1,999 | 4.8 | 7.5 | 11.9 |
| Plus $500–$999 | 8.8 | 11.7 | 16.6 |
| Plus $200–$499 | 13.5 | 14.0 | 18.1 |
| Plus $1–$199 | 21.7 | 21.9 | 19.8 |
| Zero | 6.6% | 6.0% | 7.3% |
| Negative Savings | 40.8% | 34.3% | 18.7% |
| Minus $1–$199 | 12.0% | 11.7% | 6.7% |
| Minus $200–$999 | 16.3 | 15.6 | 8.4 |
| Minus $1,000–$4,999 | 9.8 | 6.5 | 3.2 |
| Minus $5,000 or more | 2.7 | 0.5 | 0.4 |
| Total | 100.0% | 100.0% | 100.0% |
| Mean Net Savings | −$210 | $180§ | $605 |

* This table and the following ones are based on data for 3,500 spending units gathered in the 1950 Survey of Consumer Finances conducted by the Survey Research Center of the University of Michigan for the Board of Governors of the Federal Reserve System. For a discussion of the Survey see "Methods of the Survey of Consumer Finances," *Federal Reserve Bulletin*, July, 1950.

† Net Worth I is the definition of savings regularly used in the Survey of Consumer Finances. For the items included in this definition see "Distribution of Consumer Saving in 1949," *Federal Reserve Bulletin*, November, 1950.

‡ The only difference between Net Worth II and Net Worth I is that the former includes as positive saving purchases of durable goods.

§ The sampling error (two sigmas) of the estimate of mean net savings by the Net Worth I definition is $60.

[3] See "Distribution of Consumer Saving in 1949," *Federal Reserve Bulletin*, November, 1950.

[4] This definition was developed by James N. Morgan, to whom the authors are indebted for the distribution in Table 1. The durable goods included are cars, furniture, radios, television sets, refrigerators, washing machines, and stoves.

[5] A definition which does make certain allowances for depreciation is shown in Appendix II of the article mentioned in n. 3 above.

[6] The items included in Deflationary Savings are: changes in holdings of liquid assets (bank accounts and government bonds), changes in consumers' indebtedness (mortgages, instalment debt, personal loans), premium payments to and lump-sum settlements from life insurance companies, payments to retirement funds, inheritances and gifts of cash and bonds, and changes in holdings of stocks and bonds of publicly held corporations.

the price of the property or the additions (consumers' investment) is not entered, while under Net Worth I both entries are made.

In all statistics on savings in the consumer sector a critical problem is the separation of the activities of farmers and owners of unincorporated businesses in their capacities as consumers and as entrepreneurs. Deflationary Savings excludes money put into or withdrawn from such businesses and purchases of farm machinery. By excluding these items from Deflationary Savings, they are by definition included in consumer spending. Hence consumer spending per se is overstated. Deflationary Savings does include changes in total liquid asset holdings of entrepreneurs who do not keep separate accounts for their businesses.[7]

Table 3 shows the shares of Deflationary Savings attributable to occupation groups in 1949. The two entrepreneurial groups dominate the table. In part, this result may be attributed to the imperfect success of the attempt to eliminate their entrepreneurial activities. But the data also suggest that these two groups, owing to the wide fluctuations in their incomes from year to year, account for a large share of the savings in the bona fide consumer sector. Table 4 compares the con-

TABLE 2

RELATION BETWEEEN NET WORTH SAVINGS I AND DEFLATIONARY SAVINGS
(Percentage Distribution of Spending Units)

| Net Worth Savings I (Excludes Durables) | Deflationary Savings | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Plus $2,000 or More | Plus $1,000–$1,999 | Plus $500–$999 | Plus $200–$499 | Plus $1–$199 | Zero Savings | Minus $1–$199 | Minus $200–$999 | Minus $1,000–$4,999 | Minus $5,000 or More |
| Has Positive Savings | 80.4% | 97.7% | 97.6% | 97.9% | 98.6% | 9.3% | 14.9% | 14.0% | 28.0% | 48.1% |
| $2,000 or more | 62.9 | 10.4 | 4.9 | 1.5 | 0.6 | * | 0.9 | 1.6 | 4.0 | 8.9 |
| $1,000–$1,999 | 3.4 | 84.7 | 10.4 | 3.3 | 2.2 | 0.9 | 1.3 | 1.5 | 7.0 | 10.4 |
| $500–$999 | 3.8 | 0.5 | 80.8 | 13.2 | 2.9 | 1.0 | 3.4 | 3.2 | 6.5 | 13.0 |
| $200–$499 | 5.9 | 1.2 | 1.0 | 79.1 | 5.2 | 3.6 | 4.7 | 2.7 | 4.4 | 5.2 |
| $1–$199 | 4.4 | 0.9 | 0.5 | 0.8 | 87.7 | 3.8 | 4.6 | 5.0 | 6.1 | 10.6 |
| Has Zero Savings | 1.4 | * | * | * | * | 90.0 | * | * | * | 0.9 |
| Has Negative Savings | 18.2 | 2.3 | 2.4 | 2.1 | 1.4 | 0.7 | 85.1 | 86.0 | 72.0 | 51.0 |
| $1–$199 | 0.6 | 0.9 | * | 0.8 | 0.2 | * | 84.9 | 4.8 | 5.1 | 4.8 |
| $200–$999 | 5.8 | 0.9 | 1.2 | 1.0 | 0.9 | 0.3 | 0.2 | 79.8 | 13.7 | 14.7 |
| $1,000–$4,999 | 11.5 | 0.5 | 1.2 | 0.3 | 0.3 | 0.3 | * | 1.1 | 53.1 | 15.6 |
| $5,000 or more | 0.3 | * | * | * | * | 0.1 | * | 0.3 | 0.1 | 15.9 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| Percentage of sample | 3.5 | 4.7 | 8.6 | 13.3 | 21.6 | 6.6 | 11.9 | 16.1 | 9.4 | 2.5 |
| Number of cases | 146 | 202 | 320 | 463 | 685 | 206 | 391 | 573 | 357 | 100 |

* Less than .05 per cent.

[7] Large-scale entrepreneurs usually maintain separate accounts, while small-scale entrepreneurs often fail to do so. We believe that the amount by which consumer changes in liquid assets is exaggerated is relatively small.

centration of net savings within income groups under Net Worth I and Deflationary Savings. The calculations are shown for three groups: those whose net savings were positive, those whose net savings were negative, and all consumer spending units.

Under the Net Worth definition the high-income groups collectively were positive savers, while the low-income groups were negative savers; but under Deflationary Savings all income groups, except the highest, had negative savings.

TABLE 3

COMPARISON OF SHARE OF TOTAL DEFLATIONARY SAVINGS ATTRIBUTABLE TO OCCUPATION GROUPS

| Spending Units Ranked According to Their Occupation | Percentage of Spending Units | Share of Net Saving | |
|---|---|---|---|
| | | Net Worth I | Deflationary |
| Professional | 5.9 | + 12.0 | + 7.3 |
| Self-employed | 7.2 | + 41.5 | − 30.5 |
| Managerial | 5.5 | + 13.1 | + 10.0 |
| Clerical | 13.1 | + 13.8 | + 5.0 |
| Skilled | 30.1 | + 20.9 | − 18.9 |
| Unskilled | 9.8 | + 2.9 | − 5.5 |
| Farmers | 8.7 | + 7.0 | − 43.6 |
| Other* | 19.7 | − 11.2 | − 23.8 |
| Total | 100.0 | +100.0 | −100.0 |
| Percentage of all spending units | | 100.0 | 100.0 |

* This category includes students, housewives, protective service workers, unemployed, retired, farm laborers, and persons whose occupation was not ascertained.

The shares of savings attributable to each income decile are more equal in size under the Deflationary Savings definition than under Net Worth I.

This latter finding was of sufficient importance to require confirmation from data for a second year. The data for 1950, which are shown in Table 5, reinforce the finding that no income group dominates the statistics for Deflationary Savings.

In considering policies intended to influence consumers to save or to dissave, we must keep in mind that some savings are governed by contractual commitments. These contractual savings include insurance premiums, payments to retirement funds, and payments on mortgage principal. Other forms of savings bear little or no reference to earlier commitments and, within limits, may be made at the discretion of the consumer. We shall call the latter "discretionary savings."

Table 6 divides Deflationary Savings into contractual and discretionary components.[8] It shows the mean Deflationary Savings and mean contractual and discretionary savings for each income decile. For example, $430 is the mean Deflationary Savings of all spending units in the top income decile, which amounts to 4.4 per cent of the mean income of units in that decile. For each income decile, even the highest, mean discretionary savings were negative. The ratio of net discretionary savings to income falls with income, in contrast to the ratio of contractual savings to income, which is roughly constant.

One must proceed cautiously in drawing inferences from such tables as Table 6 as to which groups should be the principal objects of government policy. Assume that the economic situation requires increased deflationary saving. An increase in net deflationary saving can be achieved in two ways—by reducing negative saving and by increasing positive saving. The table does show that substantial reductions in negative savings can only be achieved by measures which affect spending units below the top tenth of the population. Indeed, if no spending

[8] It should be pointed out that net worth savings also can be divided into contractual and discretionary components.

## TABLE 4

COMPARISON OF SHARE OF TOTAL SAVINGS IN 1949 ATTRIBUTABLE TO INCOME DECILES UNDER TWO DEFINITIONS OF SAVING
(Percentage of Total Accounted for by Each Tenth)

| Spending Units Ranked According to Their Income | Savings of Net Positive Savers | | Savings of Net Negative Savers | | Savings of All Spending Units | |
|---|---|---|---|---|---|---|
| | Net Worth | Deflationary | Net Worth 1 | Deflationary | Net Worth 1 | Deflationary |
| Highest tenth | 46.6 | 38.4 | 8.8 | 16.9 | 104.7 | + 20.3 |
| Second tenth | 15.4 | 14.8 | 8.6 | 13.8 | 25.9 | − 12.2 |
| Third tenth | 10.3 | 10.6 | 8.2 | 12.1 | 13.5 | − 14.7 |
| Fourth tenth | 7.7 | 8.0 | 7.6 | 9.5 | 7.8 | − 11.9 |
| Fifth tenth | 6.7 | 7.3 | 10.4 | 8.8 | 1.0 | − 11.3 |
| Sixth tenth | 4.8 | 6.1 | 7.9 | 6.4 | 0.2 | − 7.0 |
| Seventh tenth | 3.7 | 6.6 | 8.9 | 7.1 | − 4.4 | − 8.0 |
| Eighth tenth | 2.4 | 3.4 | 9.0 | 7.9 | − 7.8 | − 15.7 |
| Ninth tenth | 2.0 | 2.7 | 7.1 | 4.9 | − 5.8 | − 8.6 |
| Lowest tenth | 0.4 | 2.1 | 23.5 | 12.6 | − 35.1 | − 30.9 |
| Total | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 |
| Aggregate savings (in billions) | $23.2 | $19.0 | −$14.0 | −$30.0 | $9.2 | −$11.0 |
| Percentage of all spending units | 59.7 | 52.6 | 34.3 | 40.8 | 100.0 | 100.0 |

## TABLE 5

SHARES OF DEFLATIONARY SAVINGS BY INCOME DECILES FOR 1949 AND 1950

| Spending Units Ranked According to Their Income | Percentage of Total Accounted for by Each Tenth | | | | | |
|---|---|---|---|---|---|---|
| | Savings of Net Positive Savers | | Savings of Net Negative Savers | | Savings of All Spending Units | |
| | 1949 | 1950 | 1949 | 1950 | 1949 | 1950 |
| Highest tenth | 38.4 | 39.0 | 16.9 | 28.2 | 20.3 | − 4.9 |
| Second tenth | 14.8 | 13.8 | 13.8 | 11.9 | − 12.2 | − 8.0 |
| Third tenth | 10.6 | 9.3 | 12.1 | 10.0 | − 14.7 | − 11.6 |
| Fourth tenth | 8.0 | 10.6 | 9.5 | 11.0 | − 11.9 | − 11.8 |
| Fifth tenth | 7.3 | 7.7 | 8.8 | 10.0 | − 11.3 | − 15.0 |
| Sixth tenth | 6.1 | 4.5 | 6.4 | 8.8 | − 7.0 | − 17.8 |
| Seventh tenth | 6.6 | 4.3 | 7.1 | 6.2 | − 8.0 | − 10.4 |
| Eighth tenth | 3.4 | 5.0 | 7.9 | 4.1 | − 15.7 | − 2.0 |
| Ninth tenth | 2.7 | 2.3 | 4.9 | 2.6 | − 8.6 | − 3.3 |
| Lowest tenth | 2.1 | 3.5 | 12.6 | 7.2 | − 30.9 | − 15.2 |
| Total | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 |
| Aggregate deflationary saving (in billions) | $19.0 | $23.3 | −$30.0 | −$34.1 | −$11.0 | −$10.8 |
| Percentage of all spending units | 52.6 | 54.8 | 40.8 | 38.3 | 100.0 | 100.0 |

unit in the top 10 per cent were a negative saver, that alone would reduce negative saving by only one-sixth.

Though negative saving can be reduced only to zero, positive saving can be increased considerably. Especially for limited periods of time, positive savings may represent a large fraction of total income. Hence, one cannot infer from the fact that positive savers in the top income decile accounted for 38 per cent of all positive saving that units in that decile should account for 38 per cent of any increase in positive saving. A judgment as to what the proper proportion would be cannot be formed on the basis of data here presented.

The data for 1950 shown in Table 5 also confirm the finding of substantial negative Deflationary Savings. Although consumers as a whole increased their net worth in 1949 and in 1950, at the same time they purchased goods and services in excess of their money incomes to an extent of an estimated $11 billion. (The $11 billion represents the difference between the savings of positive savers of $19 billion and savings of negative savers of $30 billion in 1949. For 1950 the estimates are $23 billion and $34 billion, with the difference again minus $11 billion.)

The estimate of minus $11 billion should be contrasted with the estimate of "personal saving" made by the Department of Commerce of plus $6 billion for 1949 and plus $11 billion for 1950. It should also be contrasted with the plus $9 billion and plus $14 billion for the two years obtained by using the Net Worth I definition from the Surveys of Consumer Finances. Both net worth definitions include as positive savings purchases of real estate, increases in assets of unincorporated businesses, and increases in productive assets of farmers.

Liquid savings, as estimated by the

TABLE 6

RELATIONSHIP BETWEEN CONTRACTUAL AND NONCONTRACTUAL ELEMENTS OF DEFLATIONARY SAVINGS FOR SPENDING UNITS WHEN RANKED BY INCOME (1949)

| Spending Units Ranked According to Their Income | Mean Contractual Savings of Each Decile* | | Mean Discretionary Savings of Each Decile | | Mean Net Deflationary Savings of Each Decile | |
|---|---|---|---|---|---|---|
| | Amount | As Percentage of Mean Income of Each Decile | Amount | As Percentage of Mean Income of Each Decile | Amount | As Percentage of Mean Income of Each Decile |
| Highest tenth | $680 | 6.9 | −$250 | − 2.5 | $430 | 4.4 |
| Second tenth | 350 | 7.0 | − 610 | −12.1 | − 260 | − 5.1 |
| Third tenth | 260 | 6.4 | − 570 | −14.0 | − 310 | − 7.6 |
| Fourth tenth | 260 | 7.3 | − 510 | −14.5 | − 250 | − 7.2 |
| Fifth tenth | 170 | 5.8 | − 410 | −13.8 | − 240 | − 8.0 |
| Sixth tenth | 140 | 5.4 | − 290 | −11.2 | − 150 | − 5.8 |
| Seventh tenth | 120 | 5.9 | − 290 | −14.1 | − 170 | − 8.2 |
| Eighth tenth | 60 | 4.0 | − 390 | −25.4 | − 330 | −21.4 |
| Ninth tenth | 50 | 4.7 | − 230 | −22.6 | − 180 | −17.9 |
| Lowest tenth | 40 | 13.4 | − 690 | −43.4 | − 650 | −40.0 |
| All units | $210 | 6.4 | −$420 | −12.8 | −$210 | − 6.4 |

* Contractual savings include insurance premium payments, payments to retirement funds, and payments on mortgage principal.

Securities and Exchange Commission, was \$2.7 billion in 1949 and \$2.5 billion in 1950. This amount is closer to the estimate of minus \$11 billion, as it should be, since liquid savings is also a type of deflationary savings. A conspicuous difference between liquid savings and our Deflationary Savings is that liquid savings covers all individuals, notably including unincorporated businesses, while Deflationary Savings largely excludes unincorporated businesses. Another difference is that only the former includes holdings of currency. It is beyond the scope of this paper to attempt the difficult task of estimating how much of the difference between the magnitudes is to be attributed to these and numerous other differences in the two sets of measurements.[9] It seems reasonable to assume that, although the estimate of minus \$11 billion might be closer to zero if all errors could be removed from it, the true figure for the portion of the economy covered by Deflationary Savings is negative.

We conclude that consumers in 1949 improved their net worth position, but they did so by building up their stocks of goods while, on balance, reducing their liquid assets and claims and increasing their debt. This behavior helped to maintain a high level of economic activity in that year. It is doubtful that consumers as a whole can continue negative deflationary saving indefinitely and correspondingly doubtful that this factor will continue indefinitely to press toward the expansion of the level of economic activity.

## SUMMARY

Both conceptually and in terms of relative magnitudes, net worth savings and deflationary savings differ significantly. For analysis of changes in the level of effective consumer demand, deflationary saving is the appropriate instrument. The use of net worth savings for this purpose is likely to lead to incomplete analysis and to correspondingly poor policy recommendations.

[9] For a brief comparison of personal savings, liquid savings, and Net Worth I, see James N. Morgan, "Individual Saving in 1947 and 1948," *American Economic Review*, XL (1950), 381–88.

# THE HUGUENOTS AND THE DIFFUSION OF TECHNOLOGY. II

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## IV. THE HUGUENOTS IN HOLLAND

Holland, with scarcely 1,000,000 population around 1685, had been experiencing for about a century an era of extraordinary economic prosperity. After freeing themselves from Spanish rule in the last quarter of the sixteenth century, the seven United Provinces (of which Holland was unquestionably the most important) had rapidly become the chief maritime nation of Europe and were carrying a good share of England's and France's trade. The Dutch had also become the rivals of the French and the English in many branches of industry, and Amsterdam had displaced Antwerp as the international financial center of western Europe. The country, moreover, had developed a special kind of mercantilism which allowed almost complete freedom in foreign trade and in the import and export of bullion. Although Calvinism had become the official religion, the Republic had long served as a haven for persecuted minorities such as the Spanish and Portuguese Jews and the Protestants from the Spanish Low Countries. In view of this rather unique liberalism in commerce and religion, it is not surprising that she opened her doors to the Huguenots who tried to escape from the persecution of Louis XIV; in view of her economic rivalry with France, it is not surprising that she actively encouraged them—provided they were self-sustaining[102]—to settle within her jurisdiction.

In May, 1681, just after the French dragoons were unleashed in Poitou, the state of Friesland resolved that all refugees settling in its territory would have every right enjoyed by the natives.[103] The Estates-General of the United Provinces on July 13, 1709, decreed that all Protestant refugees "shall be acknowledged and received for the future as our subjects; and they shall enjoy the right of Naturalization." The decree stated that the prosperity of most nations, and especially of Holland, had been greatly enhanced by the flood of immigrants who had sought sanctuary from the wrath of the French king and who had contributed mightily to the increase of trade, manufactures, and the common good.[104] Even before this, according to the French ambassador D'Avaux at The Hague, the Estates-General had voted 150,000 livres as aid to refugee army officers and in June, 1687, had agreed to appropriate each year 30,000 livres for their salaries.[105] The Dutch ambassador at Paris

[102] Bibliothèque Nationale, *Fonds français*, MS 7044, fol. 109. Professor James Westfall Thompson has argued that in this connection the Dutch were almost entirely motivated by their economic self-interest and that "religious sympathy was never more than niggardly given" ("Some Economic Factors in the Revocation of the Edict of Nantes," *American Historical Review*, XIV [1909], 45).

[103] Poole, *op. cit.*, p. 35. This probably was the first official act of a foreign power designed to aid the harassed Huguenots.

[104] F. P. Labillière, "Incidents of Huguenot History in the Reign of Queen Anne," *PHSL*, IV, 70-71; J. Arnal, "De l'influence des réfugiés français aux Pays-Bas," *Bulletin de la commission pour l'histoire des églises wallonnes* (hereinafter cited as "*BCHEW*") (4th ser., 2d issue [Leyden, 1929]), p. 2.

[105] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Hollande," *registre* 151, fol. 65.

secretly distributed among the Protestants in that city a small brochure in which he enumerated the advantages offered them by the Low Countries, the proper roadways to follow to elude frontier guards, and the best precautions to take to keep from being detected.[106]

Dutch cities appear to have vied with one another in trying to attract the greatest number of Huguenots. Amsterdam is said to have constructed a thousand dwellings to be let at minimal rentals to the immigrants,[107] and in September and October, 1681, she declared that all refugees within her jurisdiction would be able to enjoy bourgeois status, the right to exercise their crafts freely, the exemption for three years from import and other ordinary city charges no matter how wealthy they were, access to interest-free loans for the purpose of acquiring the tools of their trade, and an assured market for their output as long as they needed public assistance.[108] On October 7, 1681, the city magistrates voted to grant 10,000 guilders to skilled craftsmen who needed immediate financial assistance[109] and on December 13, 1682, entered into a provisional contract with one Pierre Bayle which was so liberal as to deserve special mention.[110] The city agreed to provide Bayle (or Baille) with a house suitable for forty looms, to give him 300 florins to furnish his own living quarters, to lend him forty beds, either to advance him 400 florins for one year without interest for the upkeep of his workmen or to lend him up to 3,000–4,000 florins without interest but under suitable bond, and to expand his works later on if need arose.[111] Bayle, on his part, was to make certain kinds of silk cloth not theretofore produced in Holland and to hire only Huguenot refugees unless he could prove there was an inadequate number of them to staff his manufactory. Probably no other immigrant entrepreneur received as many privileges and subsidies as did Bayle, but many of them were favored in one way or another.[112]

Nearly all Dutch cities followed Amsterdam's example and tried to attract their share of immigrants by granting them freedom to practice their arts and other similar rights.[113] Native Dutch artisans and workers, facing new competition, soon began to resent the tax exemptions and other special privileges accorded the newcomers and complained that they themselves could not find work.[114] Despite such friction, however, the magistrates were unwilling to see any of the Huguenots return to France. D'Avaux wrote Louis XIV on March 20, 1687, that, although there were many Frenchmen in Holland who wanted to return, they dared not let their wishes be known lest they be arrested by the police. The

[106] Douen, *op. cit.*, II, 431.

[107] Solomiac, "Le Réfuge dans le pays de Vaud (1685–1860)," *BSHPF*, IX, 360.

[108] C. Weiss, *Histoire des réfugiés*, II, 6–7.

[109] Leonie von Nierop, "Stukken Betreffende de Nijverheid der Refugiés te Amsterdam," *Economische-Historisch Jaarboek*, VII ('S-Gravenhage, 1921), 155.

[110] *Ibid.*, pp. 160–61.

[111] A short time later the city advanced him 10,000 guilders more without interest (*ibid.*, pp. 173–75).

[112] *Ibid.*, VII and IX (1923), *passim*. The city council was informed in July, 1690, that 1,708 refugees were receiving financial aid from the city and that the number was still growing. The council then decided to rescind all tax-exempt privileges except for those who had just arrived (*ibid.*, IX, 193–95).

[113] W. E. J. Berg, *De Réfugiés in de Nederlanden, na de Herroeping van het Edict van Nantes* (Amsterdam, 1845), pp. 33, 187, 193.

[114] *Ibid.*, pp. 161 ff.; Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Hollande," *registres* 145, fol. 26; 146, fol. 158.

Dutch were even threatening, he continued, to search all ships headed for France in their efforts to prevent repatriation.[115]

How many Huguenots found an asylum in the United Provinces? Again, as for England and Ireland, it is impossible to give a precise and accurate answer. Between 1681 and March, 1684, some 1,500 arrived in Amsterdam with about 226 looms.[116] Only 428 of them were registered as citizens before the end of 1684, and Amsterdam added over 1,700 refugees to its citizenship roll in 1685–91.[117] A Huguenot spy, in the employ of the French ambassador, wrote on January 28, 1687, that during the last six months of 1686, according to church records, over 1,800 had arrived in various Dutch cities.[118] The same individual had reported on April 15, 1686, that there were up to 16,000 refugees in the country.[119] A member of the French embassy wrote Paris toward the end of 1685 that there were already "more than" 5,000 in Rotterdam and an even larger number in Amsterdam.[120] Protestant crews of French vessels putting into Dutch ports deserted ship en masse;[121] French pastors exiled from Normandy reported that they were ministering to the spiritual needs of up to two-thirds of their old congregations;[122] each ship arriving in Holland unloaded scores of refugees;[123] and entire sections of The Hague, Rotterdam, and Amsterdam were peopled with Frenchmen.[124] There can be no doubt that by 1715 the total number of Huguenots in Holland was quite large and probably larger than the number permanently settled in any other country. Fragmentary data like the foregoing statements and inferences drawn from contemporary records create the impression that the number may have fallen between 50,000 and 75,000.[125]

The Dutch, remembering the success Colbert had achieved earlier in transplanting certain branches of the textile industry by importing Dutch workmen and copying their techniques, hoped to get revenge by encouraging the Huguenots to initiate production along lines for which France had become famous. And to a considerable extent they were successful. Many of the refugees—including Pierre Bayle—who arrived in Amsterdam before and after 1685 were subsidized to manufacture silk, especially black and lustrous taffetas.[126] Immi-

[115] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Hollande," *registres* 143, fol. 180; 150, fols. 231–33.

[116] Van Nierop, *op. cit.*, VII, 180–95.

[117] *Ibid.*, IX, 210.

[118] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Hollande," *registre* 153, fols. 15–16.

[119] *Ibid.*, *registre* 149, fol. 139.

[120] *Ibid.*, *registre* 143, fol. 167.

[121] Thomas Miller Maguire, "Frenchmen on the Seas," *PHSL*, VIII, 168.

[122] Jean Bianquis, *La Révocation de l'édit de Nantes à Rouen, essai historique* (Rouen, 1885), p. xciv.

[123] Archives du Ministère des Affaires Étrangères, *Ancient fonds*, "Hollande," *registres* 150, fol. 236; 152, fol. 174.

[124] Charles Weiss, "Mémoire sur l'influence littéraire des réfugiés protestants de France en Hollande," *Séances et travaux de l'Académie des sciences morales et politiques*, XXV, 309.

[125] Both Weiss (*ibid.*, XXV, 309) and Rebélliau (*op. cit.*, VIII, Part I, 343) state that the French ambassador at The Hague reported in 1686 that there were 75,000 refugees already in Holland. I was unable to find any such figure in the official correspondence, but I did discover an estimate of 16,000 made by D'Avaux's spy in 1686. Poole (*op. cit.*, p. 169) gives as his estimate 100,000; Cowell (*op. cit.*, p. 11) accepts 75,000; and the Abbé Caveirac, whom one would not suspect of exaggeration, is said by Weiss to have suggested 55,000 (Weiss, "Mémoire sur l'influence littéraire ...," *op. cit.*, XXV, 309).

[126] Berg, *op. cit.*, pp. 166–68; Van Nierop, *op. cit.*, VII, 158, 162, 163–65; IX, 164–65, 167–68. Van Nierop concludes that the taffeta industry in Amsterdam kept 1,000 looms busy (*ibid.*, IX, 177–81).

grants from Nîmes and Tours[127] opened important silk workshops in Haarlem and Utrecht, and Haarlem was said soon to have had from 15,000 to 20,000 workers employed in the industry[128] and Utrecht to have had a famous manufactory employing 500 people.[129] One refugee developed an especially strong silk gauze[130] and another introduced the cherished French secret of dyeing scarlet silks.[131] It is worth noting that English customs officials in March, 1690, acknowledged their error of mistaking some Dutch silk exported to England for French silk.[132] This would indicate that the refugees in Holland were successful in turning out fine-quality fabrics. A high French official complained that the Dutch were imitating the rich silk brocades for which France was renowned,[133] and the intendant at Lyons in his 1699 memorandum prepared for the edification of the young Duke of Burgundy stated that, whereas formerly the Dutch imported large amounts of black taffetas from that city, now they bought just enough to serve as models and patterns for their own manufactories.[134] One last bit of evidence may suffice to demonstrate the success achieved in silk-making. The Prussian ambassador at The Hague in 1686 reported to the Great Elector in Berlin that the French shops in the United Provinces had achieved "a prodigious success" and were copying the lustrous black taffetas of Tours and Lyons. The price of silk fabrics on Dutch markets, consequently, had fallen by 28 per cent.[135] Despite its auspicious beginning and early lusty growth,[136] the silk industry apparently declined during the latter half of the eighteenth century, and French producers once again captured the Dutch market.[137]

Somewhat the same thing happened in velvet-making. Refugees at Utrecht and Haarlem started the manufacture of rich velvets and flowered plushes, which gained favorable reputations in Dutch and foreign markets. French manufacturers at Amiens even began to sell their output as "Utrecht velvets" in order to compete with the new shops. Before the end of the eighteenth century, however, the Dutch industry was declining.[138]

In his instructions to his special envoy on December 20, 1685, Louis XIV asked Bonrepaus to learn why the Dutch had lately ceased to buy as much French linen as they formerly did for their East Indian trade and to recommend action which could possibly correct the situation.[139] Louis suggested that it might be

[127] Léon Dutil, "L'Industrie de la soie à Nîmes jusqu'en 1789," *Revue d'histoire moderne et contemporaine*, X (1908), 321–22; Archives Nationales, G⁷ 1701 (*registre*, fol. 136).

[128] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 18; C. Landré, "Souvenirs du Réfuge," *BCHEW* (1st ser., 2d issue), p. 393.

[129] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 18.

[130] Landré, *op. cit.*, *BCHEW* (1st ser., 2d issue), p. 393.

[131] Archives Nationales, *Archives de la Marine*, B² *registre* 61, fol. 219. Colbert de Seignelay wrote D'Avaux that His Majesty was willing to bribe handsomely the worker into returning to France if D'Avaux established the fact that the man actually was using the secret process and that his manufactory would close down upon his departure.

[132] Van Nierop, *op. cit.*, IX, 186.

[133] Bibliothèque Nationale, *Fonds français*, MS 7044, fols. 279 ff.

[134] *Ibid.*, *Fonds français*, MS 4288, fol. 117.

[135] Quoted by Weiss, *Histoire des réfugiés*, II, 136.

[136] It is true that high protective tariffs aided their efforts, but the refugees certainly were instrumental in diffusing the technology of silk-making and in initiating many Dutch enterprises. Natalis Rondot (*Les Protestants à Lyon au dix-septième siècle* [Lyons, 1891], p. 64, n. 1) thinks otherwise.

[137] Berg, *op. cit.*, p. 293.

[138] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), pp. 17–18; Berg, *op. cit.*, p. 208; L. Rossier, *Histoire des protestants de Picardie, particulièrement de ceux du département de la Somme* (Amiens, 1861), p. 251.

[139] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Angleterre," *registre* 157, fol. 16.

desirable to improve the quality of French linen or to persuade the French refugees who had set up shop in the United Provinces to return home. There can be no doubt that many Huguenots carried the skill and knowledge they had gained in linen and sailcloth establishments in Picardy and Provence to a large number of Dutch cities. Louis Crommelin, who did so much for the Irish linen industry, had first taken refuge in Holland, where he and other members of his family had built up thriving enterprises. More than twenty linen shops, producing the heaviest duck for sails or the finest fabrics for clothing, were opened by refugees in Haarlem. Along with others located elsewhere, they successfully exported their output as "French linens"; and, unlike silk and velvet, the manufacture of linen continued to be an important part of the Dutch economy.[140]

D'Avaux, on May 9, 1686, wrote Colbert de Seignelay that some Huguenots had recently opened a factory at Rotterdam which turned out coarse woolen cloth (*draps de meunier*), which France had formerly been selling in large quantities throughout the world. Seignelay at once (May 19) urged Bonrepaus to do his utmost to persuade the workers to return home and to spare no expense, for "this affair is close to His Majesty's heart." He reminded Bonrepaus that one of the principal purposes of his visit to Holland was to effect the repatriation of as many French workmen as possible, for "nothing is more capable of ruining the commerce of His Majesty's subjects than the establishment of our manufactures in Holland."[141] Amsterdam subsidized some Protestants who promised to make serge,[142] and a French inspector of manufactures in 1708 complained that serge manufacturers in Lower Normandy had lost their market in Holland after the refugees had set up their own looms there.[143] There is some evidence that the Huguenots also engaged in lacemaking, stocking-knitting, calico-printing, drawing gold and silver thread, and weaving rugs and tapestries.

In addition to textiles and closely allied products, Protestant immigrants from Normandy transplanted, to Rotterdam especially, their skill in making hats from beaver fur and other materials.[144] Louis XIV, Seignelay, and D'Avaux evidenced as much concern over the loss of this manufacture as they did over linen, and the Prussian ambassador at The Hague stated that the price of beaver hats in Holland had fallen by 40 per cent since the new shops were started.[145] To encourage the enterprise, the Dutch abolished all export duties on hats, raised import duties by 40 per cent,

[140] Bibliothèque Nationale, *Fonds français*, MSS 7044, fols. 279 ff.; 20967, fol. 4; Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), pp. 17–18; Berg, *op. cit.*, p. 170; Weiss, *Histoire des réfugiés*, II, 140.

[141] Archives Nationales, *Archives de la Marine*, B[7] *registre* 57, fol. 395; Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Angleterre," *registre* 160, fol. 270.

[142] Van Nierop, *op. cit.*, IX, 161–62.

[143] Cited from records in the Archives Départementales du Calvados by J.-A. Galland, *op. cit.*, p. 257. In denying the validity of the statement of some authors to the effect that the serge and woolen industries at Leyden prospered after Huguenots from Sedan and elsewhere arrived there, P. de Ségur-Dupeyron quotes figures taken from the records of the "halls and markets" of Leyden to show that the number of pieces of serge made there declined drastically from 1668 to 1708, while the output of two other kinds of woolens (*draps* and *bayettes*) increased somewhat but not phenomenally (*op. cit.*, II, 440).

[144] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 17; Van Nierop, *op. cit.*, IX, 174–76.

[145] Weiss, *Histoire des réfugiés*, II, 136, 137; Archives Nationales, *Archives de la Marine*, B[7] *registre* 57, fol. 395; Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Angleterre," *registre* 160, fol. 270.

and rescinded the old rule which restricted the number of employees of any one entrepreneur to eight.[146]

Papermakers from Angoulême and Auvergne also found employment in Dutch mills, and the French ambassador was concerned lest Dutch printers become accustomed to doing without French paper altogether, because they would have access to the output of the refugees.[147] The French deputies of trade on two separate occasions in 1708 confirmed the belief that the Huguenots had succeeded in establishing several mills in Holland and of imitating "only too well" some of the better grades of paper suitable for playing cards, fine books, and reproductions of etchings and prints.[148] The consequence, according to one author who has studied the industry in Auvergne, was that the Dutch acquired superiority over the French in paper for the last use and enjoyed a virtual monopoly in its manufacture "for more than a century."[149]

Many printers fled Lyons and similar centers of the book trade in France and set up their presses in Holland.[150] The Dutch also hoped to acquire the special techniques involved in making Norman crown glass and crystalware, but it is not clear how many of the Protestant glassblowers who planned to go there were able to avoid detection at the border.[151] Some writers have claimed that the Dutch brewing and sugar-refining industries were also strengthened by the manpower and skills provided by immigrants.[152]

Finally, the shipping and whaling industries deserve mention. If one may infer from the great concern evidenced by officers in the French marine and from the extensive efforts taken to patrol all seafaring ports and ships leaving France, large numbers of Protestant sailors and fishermen must have sought refuge in Holland following 1681. Seignelay and Louis XIV were greatly distressed lest the French navy become understaffed and her shipping and fishing fleets decline.[153] With the information at present available, it is impossible to ascertain just how much the Dutch navy, merchant marine, and fishing fleets profited from this migration. The figures for the number of Dutch whalers off the coast of Greenland each year, however, reveal that the average for 1681–85 was slightly more than 50 per cent greater than the average for 1676–80 and that the number did not decline very much until 1690.[154] The lists of personnel and directors in the whaling companies suggest that the Huguenots at least participated in the expansion.[155]

From the foregoing survey it is apparent that French Protestants affected a great variety of Dutch industries. Holland's economy, however, was primarily

[146] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 17; Weiss, *Histoire des réfugiés*, II, 137 and n. 2.

[147] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 19; Berg, *op. cit.*, p. 172; Weiss, *Histoire des réfugiés*, II, 145–46.

[148] Archives Nationales, G⁷ 1691, 1692.

[149] Michel Cohendy, *Note sur la papeterie d'Auvergne antérieurement à 1790* (extract of the *Mémoires de l'Académie de Clermont*, 1862), p. 7.

[150] Berg, *op. cit.*, pp. 175 ff.

[151] Archives Nationales, G⁷ 493; TT 430, document 100; *Archives de la Marine*, B³ *registre* 62, fols. 126–27.

[152] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 19; Berg, *op. cit.*, p. 209.

[153] See, e.g., the correspondence preserved in registers 42–62 (series B²) and 48–57 (series B³) of the *Archives de la Marine*, now housed in the Archives Nationales at Paris.

[154] Anderson, *op. cit.*, IV, 693 *bis*. Anderson's figures differ somewhat from those given by Weiss (*Histoire des réfugiés*, II, 143) and Arnal (*op. cit.*, *BCHEW* [4th ser., 2d issue], p. 19), but all implant the same impression of growth.

[155] Berg, *op. cit.*, p. 210.

commercial rather than industrial; and with the return of peace, especially after the Treaty of Utrecht, and the reopening of normal trade routes many of the new manufactories and branches of industry which the immigrants had introduced either failed or ceased to expand when confronted with competition from France. Two Dutch historians who have concerned themselves with the influence exercised by the Huguenots upon their country's growth have concluded that in the long run the effects on her industries were not very pronounced.[156] Furthermore, it is generally recognized that Holland's development in the eighteenth century did not maintain the tempo it had built up during the seventeenth century and that relative to other European countries she lost ground. She still remained, however, the chief commercial and financial center; and in these spheres the Huguenots may have exercised their greatest and most lasting influence.

Many contemporary writers agreed that the most prosperous merchants and the wealthiest among the Protestants fled to Holland. Ambassador D'Avaux wrote Louis XIV on October 23, 1685, that several rich merchants of La Rochelle, Dieppe, Rouen, and Lyons either had already arrived in various Dutch cities or had made plans to come as soon as they succeeded in smuggling their wealth out of France.[157] Exactly two years later he complained that from all appearances the richest Huguenots still left in France were planning a mass emigration to Holland and that capitalists in Amsterdam were already irked because they were finding it increasingly difficult to lend their money at more than 2 per cent interest.[158] The city itself found it could borrow at 3 per cent in 1684[159] instead of at 4 per cent, as it had done before 1679, and the interest it paid its refugee creditors was said to be about 150,000 florins a year.[160] The interest rate paid the Bank of Amsterdam by the East India Company fell from 4 per cent prior to 1685 to $3\frac{1}{2}$ per cent from around 1685 to 1723 and then to $2\frac{1}{2}$ per cent.[161] Deposits in the bank rose from an average of about 5,950,000 guilders in 1676–80 to an average of 13,750,000 guilders in 1696–1700, while its specie reserves rose from an average of 4,502,787 guilders to an average of 11,444,887 guilders. The number of depositors increased about one-third between 1671 and 1701.[162] Although it would be unwise to attribute all this to the revocation of the Edict of Nantes, one may infer that the Huguenots arriving in Holland increased the supply of liquid capital appreciably and helped bring the interest rates down.[163]

### V. THE HUGUENOTS IN GERMANY

As in the case of Ireland, the technological gap separating Germany from

[156] *Ibid.*, pp. 260 ff.; Van Nierop, *op. cit.*, VII, 150.

[157] Archives Nationales, TT 430, document 100.

[158] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Hollande," *registre* 152, fols. 107–9. Around 1700 one wrote that one could get only 3 per cent on one's money in Holland (Bibliothèque de la Société Protestante, MS 617 N, p. 73).

[159] One writer has given the rate as 2 per cent (Eugène Bersier, *Quelques pages de l'histoire des Huguenots* [Paris, 1891], p. 183).

[160] Arnal, *op. cit.*, *BCHEW* (4th ser., 2d issue), p. 5. At 3 per cent, this would mean that the Huguenots had lent the municipal government 5,000,000 florins.

[161] J. G. van Dillen, "The Bank of Amsterdam," *History of the Principal Public Banks*, collected and edited by Van Dillen (The Hague, 1934), p. 95.

[162] J. G. van Dillen, *Bronnen tot de Geschiedenis der Wisselbanken* (*Amsterdam, Middelburg, Delft, Rotterdam*) ('S-Gravenhage, 1925), II, 963–65, 985.

[163] Professor E. J. Hamilton has reminded me of the possibility that such an inference could be faulty. This I concede. The interest rate was declining at the same time in Spain and even in France, where the drain of specie, other things being equal, should have raised it. It may well have been, however, that the demand for liquid funds in France was declining more rapidly than the supply because of depressed conditions in its economy.

France was much greater than the difference between the Dutch and English economies, on the one hand, and the French economy, on the other. Germany, like Ireland, had been depopulated by warfare in the sixteenth and seventeenth centuries, and by 1685 she had become anxious to repeople her villages and farms. Certain areas were predominantly Lutheran; others were staunchly Catholic; and the ones which had an admixture of the two religions were the most generous in their treatment of the Calvinists. Since there was no true Germany but only a conglomeration of Germanic kingdoms, principalities, dukedoms, and free cities, each with its own largely autonomous government and differing public policies, it is difficult to generalize as to the government's and the public's attitude to the Huguenots, the number of immigrants who settled there, or the effect they had upon raising the level of technological practice. With the exception of Austria, Prussia (or Brandenburg) had become the foremost German state by the second half of the seventeenth century. Certainly, viewed as a refuge for the French Protestants, it was the most important. Hence most of what follows pertains chiefly to Prussia rather than to all of Germany.

Twenty-one days after Louis XIV revoked the Edict of Nantes the Great Elector Frederick William issued his famous Potsdam decree written in both French and German.[164] Since this decree became something of a model for other German rulers,[165] it will be worth while to analyze its contents in some detail.[166] The Elector ordered his agents at Amsterdam and Hamburg to provide sustenance and transportation to all Huguenots who traveled that route to Berlin or other Prussian cities where they wished to dwell. Those who chose to enter his realm via Frankfort-on-the-Main could get money, boat passage on the Rhine, and passports from his agent in that city. The immigrants could freely choose their locality, occupation or trade, and the materials they would need to set up shop or build themselves a house. The Elector promised to provide them free of charge with any ruined dwellings and enough materials to repair them or to allow them several years' free occupancy of whatever empty houses he could find provided they set to work to construct their own. They might bring with them duty-free all their furniture, personal effects, and properties useful in their trade; they were accorded all city and guild rights which natural-born citizens enjoyed; and the Elector promised to grant them all privileges and tax exemptions which would materially help them start a new manufactory and even to advance them money, tools, and raw materials to do so in so far as possible. French nobles were to enjoy noble status in their adopted country; all were to have complete freedom of worship, and the government offered to pay the salary of one minister for each town; and until they became acclimated to their new legal and social environment special judges and courts were to be established to handle all legal difficulties which would arise among them or between them and their German neigh-

[164] The decree bore the date October 29, 1685, but the German Protestant states used the Julian calendar rather than the Gregorian. There was a ten-day lag of the Julian behind the Gregorian calendar.

[165] Erbe, *op. cit.*, p. 30; Gustav Aubin, *Der Einfluss der Reformation in der Geschichte der deutschen Wirtschaft* (Halle, 1929), p. 23; Herbert W. Ellinger, *Les Réfugiés Huguenots en Saxe-Weimar; leurs manufactures de bonneterie* (Gap, 1933), p. 33.

[166] The decree may be found in the Bibliothèque Nationale, *Fonds français*, MS 7057, fols. 171 ff. It has also been reproduced in its entirety in several publications, e.g., Erbe, *op. cit.*, pp. 30 ff., and Eugène and Émile Haag, *La France protestante, ou Vies des protestants français qui se sont fait un nom dans l'histoire* ... (Paris, 1846-59), X, 465-67.

bors. Members of the French military as well as men of letters received a most cordial welcome at court and were pensioned by the government.[167]

The Landgrave of Hesse-Cassel appears to have been one of the first German princes openly to proclaim (April, 1685) that all Huguenots would find a haven in his jurisdiction,[168] and on December 12, 1685, he promulgated an edict of concessions and privileges which was similar in tone to the Potsdam decree, although somewhat less generous.[169] The Duke of Brunswick and Luneburg likewise accorded all such immigrants religious, civil, and economic freedom and promised to aid their new economic enterprises in every way possible.[170] Some rulers, like the Elector of the Palatinate, stood in fear of displeasing Louis XIV and were very cautious in their overt acts to encourage settlement in their areas.[171] Others whose provinces were strongly Lutheran or Roman Catholic took no action.

In addition to the privileges granted them by the various governments, Huguenots settling in Germany received relief aid from funds raised by public collections and also from more ample funds supplied by sympathizers in Holland, Switzerland, and England.[172] The Great Elector encouraged new enterprises by providing them with working quarters, some equipment, and loans without interest; by buying some of their output for himself and his court; and by clothing his troops in woolens produced by the refugees.[173] He and other rulers levied tariffs on foreign goods which would compete with the new domestic products and exempted raw materials from local excises.[174]

The native population did not always indorse wholeheartedly the special treatment which their rulers accorded the immigrants. In Saxe-Weimar, for example, their jealousy as well as the hostility of the Lutheran church has been cited as a reason why the French colony of manufacturers there lasted only fifteen months.[175] Even in Prussia considerable friction developed.[176] Louis XIV's representative at Berlin, Rebenac, wrote in 1686 and 1687 that the refugees were holding themselves aloof from the natives; that many of them were fast becoming disgruntled and wished to return

[167] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Prusse," *registre* 25, fols. 344–45, 426–28. The best contemporary account of the measures adopted in Prussia to aid and encourage the refugees is that written by Charles Ancillon, *Histoire de l'établissement des françois réfugiés dans les états de son altesse électorale de Brandebourg* (Berlin, 1690). The Huguenots may have provided more than one third of the officers in the Prussian army (Erbe, *op. cit.*, p. 66).

[168] Erbe, *op. cit.*, p. 29.

[169] Haag, *op. cit.*, X, 472–74; C. Couderc, "L'Abbé Raynal et son projet d'histoire de la Révocation de l'Édit de Nantes, documents sur le Réfuge," *BSHPF*, XXXVIII, 601; Bibliothèque Nationale, *Fonds français*, MS 6432, pp. 123–24.

[170] Erbe, *op. cit.*, p. 29; Bibliothèque Nationale, *Fonds français*, MS 21616, fols. 379–81. The king of Denmark did the same thing (Bibliothèque de la Société Protestante, MS 617 L, p. 250).

[171] André Paul, "Les Réfugiés huguenots et wallons dans le Palatinat du Rhin du XVI[e] siècle à la Révolution," *Revue historique*, CLVII (Paris, 1928), 264 ff.; see also Bibliothèque de la Société Protestante, MS 617 L, pp. 56–57.

[172] Erbe, *op. cit.*, pp. 40, 42; Bibliothèque de la Société Protestante, MS 617 N, p. 71; Couderc, *op. cit.*, *BSHPF*, XXXVIII, 602.

[173] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Prusse," *registre* 28, fol. 347.

[174] *Ibid.*, *registre* 27, fols. 367–68.

[175] Ellinger, *op. cit.*, p. 132.

[176] Helmut Erbe in his *Die Hugenotten in Deutschland* has attempted, among other things, to trace and to evaluate the attitude of the rulers and also of the natives to the newcomers. The Huguenots practically formed a "state within the state" until October 30, 1809, when the king abolished the last vestiges of autonomy at law, in education, in religion, and in local government.

to France; and that the Germans had developed "a prodigious aversion" to the newcomers.[177] In Dresden, Leipzig, Frankfort-on-the-Main, and Halle the refugees faced outright discrimination by local craftsmen and the Lutheran church; and most guilds in Brandenburg accepted Huguenots as members only because they were required by law to do so. Frederick William, however, was so anxious to attract and then to retain them that in 1688 he published an order throughout the land not to allow any Frenchman to leave the country unless he had a passport issued by the Elector himself.[178] This so infuriated Louis XIV that he wrote Rebenac at Berlin on February 12, 1688:

> I desire that you inform the Elector of Brandenburg that I would have had already sufficient reason to complain because of all the publications and declarations which have issued from his part to excite my subjects of the Allegedly Reformed Religion to desert and retire to his states, but that I will not suffer that he retain by force those who see the error of their ways and wish to return to my realm; and that, if this violence continues, it will indeed cause me to take certain measures which will not be agreeable to him.[179]

How many Huguenots actually settled in Germany? Because of certain censuses taken in the eighteenth century and also because of careful research by German historical scholars, there is less uncertainty and divergence of opinion for Germany than for any other host area. Of the approximate 30,000 who migrated to all German provinces, about two-thirds found haven in Brandenburg, which had a total population of about a million or a million and a half. Almost 6,000 of these founded the "new city" outside old Berlin or settled within the walled area itself. Hence they constituted about one-fifth or one-sixth of the capital's population. Perhaps 2,000–3,000 settled in Hesse-Cassel; approximately 2,000 in Luneburg; 1,000–2,000 in Bavaria; and the rest in other provinces. About 40 per cent passed up the Rhine, and the remainder came via Holland.[180]

About 70 per cent of the immigrants were between twenty and fifty years of age and represented a great diversity of occupations. The list prepared in 1700 of those in Prussia reveals that 88 were apothecaries, doctors, architects, or opticians; 164 were teachers, students, notaries, preachers, artists, and jewelers; 409 were tradespeople (bakers, distillers, brewers, confectioners, barbers, wigmakers, and merchants); 169 were civil and domestic servants; 213 were engaged in gardening, raising tobacco, and farming ("countrypeople"); 540 were craftsmen;[181] 46 were listed as workpeople and 34 as landlords; 11 made gauzes or printed cloth; 35 were engaged in hatmaking and 14 in button-making; 93 were tailors; 739 were employed in the lace, silk,

[177] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Prusse," *registres* 26, fols. 75–76, 194–95; 28, fols. 347–48; 29, fols. 56–57; 30, fol. 94.

[178] *Ibid.*, *registre* 30, fol. 54.

[179] *Ibid.*, *registre* 33, fol. 18.

[180] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Prusse," *registre* 28, fol. 348; *ibid.*, "Suisse," *registre* 81, fol. 384; Bibliothèque Nationale, *Fonds français*, MS 6432, pp. 122, 139; Anderson, *op. cit.*, II, 568; Couderc, *op. cit.*, *BSHPF*, XXXVIII, 592 ff., 641; Erbe, *op. cit.*, pp. 34, 39, 255–56, 262–65; Erman and Reclam, *op. cit.*, II, 35–38; VI, 134; P. Sagnac, in Lavisse's *Histoire de France*, VIII, Part I, 207; Novi de Caveirac, *Apologie de Louis XIV*, pp. 86–87; *PHSL*, XVI, 266–68; C. Weiss, *Histoire des réfugiés*, I, 138. As usually happened, Louis XIV's representative at Berlin attempted to minimize the number of refugees.

[181] This number included blacksmiths, bookdealers and printers, brass-founders, candlers, coopers, coppersmiths, cutlers, engravers, furriers, glassworkers, glaziers, joiners, locksmiths, masons, mirror-makers, needle- and pinmakers, papermakers, perfumers, pewterers, potters, saddlers, soapmakers, shoemakers, sword-cutlers, tanners, tinsmiths, tobacco workers, and watchmakers.

stocking, woolen, and tapestry industries; 710 were widows, maiden ladies, or wives of army officers; and 479 either did not give their occupations or were "persons of quality."[182] In view of the fact that economic development in Germany had lagged behind that in most European countries, the refugees must have exercised a leavening influence in a great many branches of trade and industry. One of these was the manufacture of various kinds of woolen cloth in which France had developed a special proficiency.

The Elector Frederick William financed the construction of several fulling mills, presses, and dyeing shops; advanced individuals some operating capital; forbade the export of raw wool and taxed woolen imports; ordered his troops to be clothed in domestic fabrics; and promulgated rules and standards to guide the manufacturers.[183] Most of the woolen shops begun by the Huguenots in Berlin, Brandenburg, Cassel, Erlangen, Frankfort-on-the-Oder, Halle, Magdeburg, and elsewhere were built on a small scale; but a few apparently employed scores of workers.[184] One refugee from Rouen, formerly employed at Gobelins, carried with him the secret of dyeing fine woolen fabrics a bright red. Others began silk mills in a few places, introduced ribbon-weaving in Königsberg, carried with them the art of printing calicoes, and introduced the manufacture of gauze into Brandenburg from Soissons.[185] Some made the oils and liquid soaps so essential in washing the wools used in the better fabrics. Several Frenchmen complained that Huguenot refugees had been so successful in their attempts to manufacture linen fabrics at Hamburg and other places in Germany that they were taking the English, Spanish, and Indian markets away from the French and imitating quite well the famous products of Saint-Quentin, Laval, and Morlaix and that they were even likely to try to sell their output in France.[186]

There were about 260 stocking workers among the Huguenots in Prussia in 1700. Apparently they introduced into Prussia the knitting frame and made large quantities of wool stockings at Magdeburg.[187] Even though the French colony in Weimar lasted only a short time, it left distinct traces on the industry there.[188] One stocking manufacturer from Languedoc who had settled in Magdeburg wrote his brother at Zurich in 1687 that he should join him without delay. "Wool is of good quality and cheap," he

[182] Erbe, *op. cit.*, pp. 268–71.

[183] Erman and Reclam, *op. cit.*, V, 10 ff.; Ancillon, *Histoire de Brandebourg*, pp. 218–19; C. Weiss, *Histoire des réfugiés*, I, 156; Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Prusse," *registre* 28, fol. 347.

[184] Bibliothèque Nationale, *Fonds français*, MSS 6432, pp. 125–27; 7044, fols. 279–83; Erman and Reclam, *op. cit.*, IV, 323, 329 ff., 340–42; Couderc, *op. cit.*, *BSHPF*, XXXVIII, 602; Édouard Sayous, "La Colonie réformée d'Erlangen en Bavière," *BSHPF*, XXXVI, 6–7.

[185] Archives Nationales, G⁷ 1701 (*registre*, fol. 136); Aubin, *op. cit.*, p. 19; Erman and Reclam, *op. cit.*, V, 120; VI, 11; C. Weiss, *Histoire des réfugiés*, I, 162–63; Edmond Chevrier, *Le Protestantisme dans le Mâconnais et la Bresse aux XVI^e et XVII^e siècles* (Mâcon, 1868), p. 68; Édouard Fleury, "Notice sur l'histoire du protestantisme dans le département de l'Aisne," *Bulletin de la société académique de Laon*, XI (1861), 257.

[186] Archives Nationales, G⁷ 1700 B (*registre*, p. 170); *ibid.*, *Archives de la Marine*, B⁷ *registre* 495, fol. 39; Boislisle, *op. cit.*, II, 489; John Sutherden Burn, *The History of the French, Walloon, Dutch, and Other Foreign Protestant Refugees Settled in England etc.* (London, 1846), p. 19. Anderson (*op. cit.*, II, 597) stated that such linen goods as dowlas and lockram, formerly the pride of Normandy and Brittany, were being produced in Hamburg.

[187] Erbe, *op. cit.*, pp. 268–71; Couderc, *op. cit.*, *BSHPF*, XXXVIII, 602; Erman and Reclam, *op. cit.*, IV, 330–31; V, 124; Bibliothèque Nationale, *Fonds français*, MS 6432, pp. 125–26, 157–58.

[188] Ellinger, *op. cit.*, pp. 109, 112–13, 132–33.

wrote. "The spinners are skillful and their wages low; the country is beautiful; food is cheap; and one easily becomes accustomed to beer."[189] Several hat manufacturers from Burgundy and Normandy began to fashion beaver, rabbit, and fine woolen hats in various places where only the coarsest variety had been made theretofore.[190] Erman and Reclam, writing in the last two decades of the eighteenth century, stated that at the time of the revocation there had been only one tanner in Berlin and that he had understood his trade only imperfectly.[191] The refugees established several tanneries and so increased the output of leather goods that King Frederick William in 1717 and 1726 forbade the wearing of wooden shoes in his cities, and the number of people who went barefooted also declined.[192] Glove factories were started at Halle, Halberstadt, and Magdeburg; and the glovemakers in Berlin, not organized along factory lines, received in 1702 a charter for their Guild of French Refugee Glovers.[193]

The Great Elector sought to encourage the weaving of fine tapestries in the Aubusson tradition, and to this end he granted letters patent in November, 1686, to one Pierre Mercier. Other Protestant refugees apparently worked in the same art, and for several decades they supplied the palaces at Berlin and Potsdam and sent some of their panels to Russia, Sweden, and Denmark. Tapestry-weaving was an industry Germany could ill afford; and it prospered only so long as the princes continued their heavy subsidies.[194] A Frenchman who had first started a plate-glass factory at Copenhagen shortly before 1690 went to a window and bottle glass shop at Neustadt and began to make blown plate glass. His son later introduced there the French method of casting large plates. The factory proved to be successful and continued operations at least until the nineteenth century.[195] Other Huguenots helped to make Hanau famous for its jewelry, and still others in 1686 founded a shop for gold and silver galloons which the king finally took over as a royal enterprise in 1737.[196] The privilege of making playing cards, it may be noted, remained in the hands of a refugee family throughout the eighteenth century.[197]

With many of its fields laid waste by a century of almost constant warfare and with ample lands that had never been cultivated, Germany offered special advantages to French peasants who, on the whole, were too poor to pay their passage to England, Ireland, or Holland or to acquire farms once they arrived and yet were somehow able to make their way up the Rhine Valley from the southern and northeastern French provinces. The Great Elector gave them land, building materials, livestock, and seed in order to

[189] Lavisse, *op. cit.*, VIII, Part I, 207.

[190] Bibliothèque Nationale, *Fonds français*, MS 6432, pp. 125–26, 157–58; Couderc, *op. cit.*, *BSHPF*, XXXVIII, 602; Erman and Reclam, *op. cit.*, IV, 330; V, 49, 54–57; C. Weiss, *Histoire des réfugiés*, I, 161; Court, *Le Patriote françois*, I, 382–83.

[191] *Op. cit.*, V, 57.

[192] *Ibid.*, p. 61.

[193] *Ibid.*, pp. 66, 69.

[194] *Ibid.*, IV, 330; V, 128, 134–35; Alfred Leroux, *Histoire de la réforme dans la Marche et le Limousin* (Limoges, 1888), pp. 331–33; Fernand de Schickler, "Le Protestantisme dans la Marche et l'Église d'Aubusson," *BSHPF*, XXX, 299–300.

[195] Erman and Reclam, *op. cit.*, V, 194–95, 203–4. It was this enterprise which caused Louis XIV in 1700 to entertain the idea of asking the Royal Plate Glass Company to open a branch factory in Alsace near Haguenau. He hoped that the resulting competition would force the works at Neustadt to close down (Warren C. Scoville, *Capitalism and French Glassmaking, 1640–1789* [Berkeley and Los Angeles, 1950], p. 35, n. 29).

[196] Erman and Reclam, *op. cit.*, V, 122–23, 272–73.

[197] *Ibid.*, pp. 78–79.

get the new agricultural colonies under way.[198] The French peasant, like his fellow-craftsman, was more advanced than his German neighbor. Not only did he bring new land under cultivation and use intensive methods of gardening which caused his farms to sparkle like jewels along the countryside, but he also introduced some new crops. He planted mulberry trees and attempted to raise silkworms; he introduced a variety of woad which was said to be especially good for various dyeing tints; he grew tobacco on a large scale; and he planted artichokes, asparagus, cauliflower, potatoes, and fine fruit trees.[199]

As has been pointed out earlier in this paper, the wealthiest Huguenots fled to England or Holland. Some, however, escaped to Germany with their wealth more or less intact, and from 1686 to 1691, it was said, those in Berlin loaned the Elector almost 90,000 thalers at 6–8 per cent interest.[200] One German historian, writing in 1751, estimated that the refugees from Metz alone introduced about 2,000,000 *écus* into Brandenburg.[201] And Frenchmen occupied important places in the administration of a loan bank set up by the Elector in 1689 and 1692.[202] Others founded important commercial houses in Berlin, Halle, and Magdeburg. Their contributions to the German economy in the fields of finance and large-scale commerce, however, fell far short of their contributions in agriculture and the crafts.

King Frederick I (or Elector Frederick III) in his edict issued at Cologne on December 14, 1709, acknowledged the benefits they had bestowed on his country:

> In general the welfare of countries consists of the multitude of their subjects, and in particular our realm and our provinces have experienced a considerable improvement by the great number of persons, who, having been chased from their homeland for religious reasons and various other oppressions, have come to seek a haven under our protection [and] by establishing a quantity of manufactures have aided industry to increase and trade to flourish—in which process particularly the refugees from France who have withdrawn into our states have greatly participated.[203]

Ségur-Dupeyron, who has argued that the Huguenot influence in England and Holland was not very great, has admitted that it had "an incontestable success from the point of view of manufacturing" in Prussia, where all industries except those associated with cloth were yet to be started in 1685.[204] Some writers think that the refugees initiated up to sixty-five trades, and Gustav Schmoller has acknowledge their "most durable and favorable influence on all the intellectual and economic development of the country."[205] As all economic historians are aware, Germany did not close the economic and technological gap which separated her from her western neighbors until well into the nineteenth century; but the Huguenots helped build the varied crafts and people her cities with traders and artisans, all of which proba-

[198] Ancillon, *Histoire de Brandebourg*, pp. 280–81.

[199] Erman and Reclam, *op. cit.*, VI, 253 ff.; Ellinger, *op. cit.*, p. 114; Paul Beuzart, "Une Famille de réfugiés de la Thiérache," *BSHPF*, LXXXIX, 266–67; Bibliothèque Nationale, *Fonds français*, MS 6432, pp. 157–58.

[200] Erman and Reclam, *op. cit.*, I, 320–24. The interest rate in German was certainly higher than in Holland (Bibliothèque de la Société Protestante, MS 617 N, p. 73).

[201] Erman and Reclam, *op. cit.*, II, 31.

[202] *Ibid.*, V, 29 ff.

[203] Mme Alexandre de Chambrier, "Naturalisation des réfugiés français à Neuchâtel de la Révocation de l'Édict de Nantes à la Révolution française, 1685–1794," *Musée neuchâtelois*, 37th year (1900), p. 201.

[204] *Op. cit.*, II, 443.

[205] *Die Preussische Kolonisation des 17. und 18. Jahrhunderts* (Leipzig, 1886), p. 15.

bly facilitated the subsequent transition from a feudal agricultural economy to a highly industrialized one. Had the Huguenots identified themselves more completely with their new neighbors and been less clannish, German economic growth in the eighteenth century would surely have been greater than it actually was.

### VI. THE HUGUENOTS IN GENEVA AND SWITZERLAND

Geneva and the Swiss cantons have been called the arch through which a flood of some 60,000 refugees surged between 1682 and 1720.[206] Of this number, perhaps 25,000 remained in Switzerland and the rest moved on to Germany, Holland, and elsewhere.[207] The influx, beginning in 1682 when rumors of impending dragonnades spread like wildfire through the French provinces, reached its peak in 1686–87. During each of several weeks in the last half of 1687 from 700 to over 1,000 migrants filed through Geneva,[208] and by the end of that year upward of 28,000 refugees probably had passed that way.[209] The inhabitants of Gex and certain valleys in Dauphiné fled almost as a body in 1685–86. The migration dwindled in the 1690's only to be followed by a mass exodus of some 3,000 individuals from the Principality of Orange in 1703, after Louis XIV began to convert the Protestants there.[210] The Swiss cantons were unable to absorb all the arrivals. Geneva had opened a French Exchange (*Bourse*) and Bern a Chamber of Refugees to aid the most necessitous newcomers and to try to find ways to resettle them in other Swiss areas or in Germany.[211] According to Tambonneau, Louis XIV's envoy to Switzerland, four cantons had already met before October 20, 1685, to consider the best way to deal with the mounting tide.[212] An official delegation, appointed in 1688, visited various German states and other countries in northern Europe to enlist their aid.[213] The Elector of Brandenburg, the Landgrave of Hesse-Cassel, and the Prince of Bayreuth, among others, rallied to the plea.[214] The Chamber of Refugees

[206] J. Gaberel, "Les Suisses romands et les réfugiés de l'édit de Nantes," *Séances et travaux de l'Académie des sciences morales et politiques*, LIV (1860), 98–99; Johann-Caspar Moerikofer, *Histoire des Réfugiés de la Réforme en Suiss*, trans. G. Roux (Paris, 1878), p. 199; Chevrier, *op. cit.*, p. 66. A Mr. Bertrand, in a memorandum written shortly before the French Revolution, stated that some 140,000 passed into or through Switzerland (Bibliothèque Nationale, *Fonds français*, MS 6432, p. 187), and Couderc (*op. cit.*, *BSHPF*, XXXIX, 48) apparently has accepted this figure. Novi de Caveirac's estimate of 12,000 (*Apologie de Louis XIV*, pp. 86–87) falls far short of any reasonable figure.

[207] Poole (*op. cit.*, p. 169) arrived at this same conclusion.

[208] Pierre Bertrand, *Genève et la Révocation de l'Édit de Nantes* (Geneva, 1935), p. 68.

[209] Solomiac, *op. cit.*, *BSHPF*, IX, 143.

[210] Eugène Arnaud, "Les Derniers jours de l'église d'Orange, 1703–1731," *BSHPF*, XXXII, 491. It has been estimated that about half of these Orange refugees fled to Geneva (Gaitte, "L'Émigration des protestants de la principauté d'Orange sous Louis XIV [1703]," *BSHPF*, XIX, 353; Moerikofer, *op. cit.*, p. 333).

[211] The records kept by these two agencies provide the most important data available as to the number and financial condition of the refugees. The Bibliothèque de la Société Protestante at Paris has a list of those assisted by the Exchange at Geneva (MS 151[1]).

[212] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registre* 80, fol. 370. The French ambassador at The Hague wrote Louis XIV on November 21, 1686, that the states of Zurich and Bern had recently addressed a letter to the Estates-General of Holland inquiring whether the Dutch provinces could help relieve the pressure of immigrants by providing permanent homes for part of the multitude (*ibid.*, "Hollande," *registre* 148, fol. 179).

[213] Bibliothèque de la Société Protestante, MS 617 L, *passim;* Mme Alexandre de Chambrier, "Projet de Colonisation en Irlande, par les réfugiés français, 1692–1699," *PHSL*, VI, 374.

[214] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registre* 81, fol. 384; Bibliothèque Nationale, *Fonds français*, MS 6432, p. 184.

on July 3, 1698, published a statement recommending that as many Protestants as possible should pass on to Germany, where they were assured of a permanent home and the right to engage in business.[215]

Of the 25,000 who may have remained in Switzerland, between 3,000 and 4,000 settled in Geneva and comprised about one-fourth its population.[216] By 1698 what is now the canton of Vaud (but in the seventeenth and eighteenth centuries was part of the state of Bern) provided a refuge for more than 6,000 Frenchmen, and its cities of Vevey and Lausanne had populations which were from one-half to one-fourth immigrant.[217] Bern itself had about 2,000 refugees or one-fifth its population.[218] From December 3, 1683, to January 1, 1689, according to an official list, 23,345 Huguenots sought assistance from Zurich, but most of them soon moved on to Germany. Perhaps from 4,000 to 8,000 remained permanently in the area.[219] The cantons of Neuchâtel and Saint-Gall also harbored some Frenchmen.[220]

Despite strong religious ties between the Swiss Protestant cantons and the Huguenots, the Swiss could not or, at least, did not proffer the refugees as warm a welcome as did the other nations already considered in this paper. Geneva, Bern (including Vaud), and Zurich may have spent as relief over a period of four decades more than 10,000,000 florins for food, clothing, and temporary shelter;[221] but they offered very little inducement for the refugees to settle there. Louis XIV so cowed Geneva with dire threats of economic and even military sanction that prior to 1688 the city refused to accept any Huguenots as "inhabitants." From 1688 to 1729 she accorded this right to only 1,235 of them and refused to grant them the full status of citizen (i.e., admit them to the *bourgeoisie*).[222] The natives complained during the economic crisis of 1693–96 that the newcomers had doubled the cost of living, had swollen the economy artificially, and were so avid of gain that many undesirable business practices had sprung up. The City Council then held eighteen public hearings "to remedy the disorder which the foreign merchants had introduced into the commerce of this city."[223]

Elsewhere the story was pretty much the same. At the outset towns advanced

[215] Moerikofer, *op. cit.*, pp. 248–49.

[216] Bertrand, *op. cit.*, pp. 106, 118; Émile Piguet, "Les Dénombrements généraux de Réfugiés au Pays de Vaud et à Berne, à la fin du XVII$^e$ siècle," *BSHPF*, LXXXII, 37–38. The censuses taken in 1693 and 1698 have proved most helpful.

[217] Solomiac, *op. cit.*, *BSHPF*, IX, 143–44; Piguet, *op. cit.*, *BSHPF*, LXXXII, 37–38; Mme Alexandre de Chambrier, *Henri de Mirmant et les réfugiés de la Révocation de l'Édit de Nantes, 1650–1721* (Neuchâtel, 1910), p. 64; Jules Chavannes, *Les Réfugiés français dans le pays de Vaud et particulièrement à Vevey* (Lausanne, 1874), pp. 25–26 n., 137–38.

[218] Bibliothèque Nationale, *Fonds français*, MS 6432, pp. 181, 184; Piguet, *op. cit.*, *BSHPF*, LXXXII, 37–38; Chavannes, *op. cit.*, pp. 25–26 n.; Mme Alexandre de Chambrier, "Les Réfugiés français en Suisse de 1593 à 1699 et la convention entre Berne et les cantons evangéliques," *BSHPF*, LVIII, 108.

[219] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registres* 80, fol. 399; 83, fols. 471–72; Moerikofer, *op. cit.*, pp. 215, 230–31; Solomiac, *op. cit.*, *BSHPF*, IX, 143; Ernest Combe, *Les Réfugiés de la Révocation en Suisse* (Lausanne, 1885), p. 67; G. Desdevises du Dézert, *L'Église et l'état en France depuis l'Édit de Nantes jusqu'au Concordat (1598–1801)* (Paris, 1907), p. 157.

[220] See, especially, Chambrier, "Naturalisation des réfugiés à Neuchâtel," *Musée neuchâtelois*, 37th year; and Théodore Rivier, *L'Église réformée française de Saint-Gall (1685 à nos jours)* (Paris, 1909).

[221] Gaberel, *op. cit.*, p. 100; Moerikofer, *op. cit.*, pp. 230–32.

[222] Bertrand, *op. cit.*, pp. 96, 98, 120, 124, 193. By not being citizens they were restricted to selling their wares only on the two fair days each week and might not keep open booths.

[223] Bertrand, *op. cit.*, pp. 109 ff.

money for new enterprises or underwrote the loans made by private individuals; some urged the guilds to open their doors to the newcomers; others received them as inhabitants or even as citizens. The Huguenots, however, proved to be almost as clannish in Switzerland as they did in Germany and remained an easily identifiable minority. As the War of the League of Augsburg disrupted the Swiss economy, the natives inevitably blamed all or part of their hardship on their newly arrived competitors, many of whom seemed to be prospering while the profit margins of older establishments were shrinking. The guilds in Zurich, for example, manifested an increasing reluctance to accept the new artisans, who probably possessed higher degrees of skill than did the older members.[224] High French officials, both in Switzerland and in Paris, stated that the refugees were increasingly unhappy in their exile because of the social and economic discrimination against them.[225]

The important question for the purposes of this study is whether the refugees initiated any new industries or diffused advanced techniques in old crafts. By and large, most of the immigrants appear to have been members of the *petite bourgeoisie* and to have engaged in such trades as baking, butchering, confectionery, glovemaking, lacemaking, printing, building, tailoring, and wigmaking or in horticulture.[226] A few industries, however, profited from their skills and new techniques. Although some doubt exists whether the French introduced the printing of cotton cloth into Geneva and other Swiss cities because the Revocation chased them from their homeland or because the French government on October 26, 1686, forbade the stamping of calicoes and the printing of all white cottons in its efforts to protect its older textile industries, there is no doubt that the Huguenots played an important part in perfecting and expanding the industry especially after 1700.[227] The factory, begun by Huguenots at Geneva around 1702, occupied from 600 to 800 persons in 1728 and as many as 2,000 in 1785.[228] Silk workers fled Nîmes and manned a manufactory at Lausanne;[229] others from the same area and from Orange began to make silk sewing thread, stockings, ribbons, fabrics, and taffetas at Geneva;[230] a silk shop was opened at Vevey in 1682;[231] and two brothers set up two silk-stocking factories at Zurich in 1686 and 1689.[232] At least at Geneva, the weaving of silk fabrics did not prove a financial success because of competition from Lyons, the war, economic blockades, and the dear-

[224] Moerikofer, *op. cit.*, pp. 246-47.

[225] Archives Nationales, G[7] 1685; Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registre* 83, fol. 281. For the references which support the generalizations as to the Swiss attitude toward the Huguenots, see Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registres* 80, fol. 399; 83, fol. 32; Moerikofer, *op. cit.*, pp. 233 ff., 250 ff.; Solomiac, *op. cit.*, *BSHPF*, IX, 145, 360; C. Weiss, *Histoire des réfugiés*, II, 214 ff.

[226] See, e.g., Bertrand, *op. cit.*, p. 187, and the list of those who settled in Vevey which is given in Chavannes, *op. cit.*, p. 139. Louis XIV's ambassador wrote on April 5, 1687, that the refugees in Switzerland from Piedmont were especially a problem because they knew only agriculture and the Swiss were not at all lacking in farmers (Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registre* 83, fol. 376).

[227] Bertrand, *op. cit.*, pp. 138 ff.; Moerikofer, *op. cit.*, p. 235; Chavannes, *op. cit.*, p. 62; Guillebert, "Le Réfuge dans le pays de Neuchâtel (1685)," *BSHPF*, IV, 154.

[228] Bertrand, *op. cit.*, p. 144.

[229] Dutil, *op. cit.*, p. 321.

[230] Bertrand, *op. cit.*, p. 99.

[231] Chavannes, *op. cit.*, pp. 63-64.

[232] Moerikofer, *op. cit.*, p. 238.

ness of materials; and by 1700 there was not a single silk mill at work in the city. But the ribbon, silk-stocking, and silk-sewing-thread branches continued there during a great part of the eighteenth century.[233] It is interesting to note, on the other hand, that the French statesman D'Aguesseau predicted at the time France was negotiating a trade treaty with Switzerland in 1714 that the Swiss would undersell domestic silk manufacturers in the French market unless import duties were collected on their products. Because labor was cheap in Switzerland and because no excises were levied on raw silk and wool imports, as was the case in France, Swiss manufacturers would have low operating costs. Furthermore, he advised, "the French refugees have perfected their industry to such a point that the Swiss, who used to buy from us all necessary items, would like to send us their products without having further need of ours."[234] D'Aguesseau also referred in the same document to woolens. The Huguenots did set up in various places a few looms for knitting stockings and for weaving serges and coarser woolens as well as some dye-houses and places where wool nap was clipped.[235]

Some writers have claimed that the French who arrived in Switzerland after 1685 were primarily responsible for the truly remarkable growth of the watch and clock industries.[236] This is somewhat misleading. As Bertrand points out,[237] there were two important waves of Protestant immigration from France: the first occurred during the fifty years preceding the Edict of Nantes (1598), and the second came a century later when the Edict was revoked. It was the first wave which profoundly affected the political, social, and economic life at Geneva and which resulted in the establishment of many new trades such as clockmaking. At the end of the seventeenth century those who sought refuge in the city, instead of acting as diffusers of technology, apprenticed themselves to watch- and clockmakers who were on the average far more skilled than they.[238] They probably participated in the industry's growth during the eighteenth century,[239] but there is little concrete evidence which allows one to assess quantitatively or qualitatively the importance of their participation. French jewelers, goldsmiths, and makers of passementerie expanded their appropriate trades in Geneva, but gilding steadily declined after the Revocation.[240] One Frenchman advised his government

[233] Bertrand, *op. cit.*, p. 131.

[234] Archives Nationales, $G^7$ 1700 C (*registre*, pp. 217–18). The Intendant at Lyons in his 1699 *Mémoire* wrote that the Swiss had learned so well to imitate French fabrics and "nearly all the different manufactures of France" that they threatened to compete with French manufacturers (Bibliothèque Nationale, *Fonds français*, MS 4288, fol. 125).

[235] Bertrand, *op. cit.*, pp. 99, 125; Gaberel, *op. cit.*, p. 110; Chavannes, *op. cit.*, pp. 63–64; Moerikofer, *op. cit.*, p. 238. Zurich denied them permission to set up laundries and dyehouses in order to protect the jobs of its own citizens already engaged in this work. And Louis XIV rejoiced to hear that the manufacture of stockings at Bern was meeting with no success (Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registres* 83, fol. 399; 84, fol. 155).

[236] Théodore Claparède, "Les Réfugiés protestants du pays de Gex," *BSHPF*, XXIV, 69; Gaberel, *op. cit.*, p. 109; C. Weiss, *Histoire des réfugiés*, II, 219.

[237] *Op. cit.*, p. 92.

[238] *Ibid.*, pp. 145–46, 188. This position is supported by Pierre Dubois, *Histoire de l'horlogerie depuis son origine jusqu'à nos jours etc.* (Paris, 1849), p. 126.

[239] Around 1685 Geneva had 100 master clockmakers and 300 workers and turned out almost 5,000 watches a year; a century later the city employed 6,000 people in the industry and made more than 50,000 watches each year (C. Weiss, *Histoire des réfugiés*, II, 219; Bertrand, *op. cit.*, p. 145).

[240] Bertrand, *op. cit.*, pp. 136–38, 148; Combe, *op. cit.*, pp. 152–53.

at the time of the projected trade treaty with Switzerland that unless tariffs were levied on Swiss goods "it will become necessary to abandon a great many of our light manufactures which our refugees, a great deal more industrious than the Swiss, have introduced into that country since they have withdrawn from the realm."[241]

Some wealthy Huguenots escaped to Switzerland with all or part of their liquid assets and invested their funds with other merchants or set up financial houses of their own. According to one writer, Geneva by 1710 was well on the road to becoming a center of finance for Central Europe.[242] More immediately important than this development in finance was the expansion of commerce, especially with foreign countries. One refugee at Zurich made a fortune from his trade in silks, cloth, and wool with Italy and with such towns as Antwerp, Amsterdam, and Nuremberg.[243] Neuchâtel gradually developed an active commerce with many parts of the world,[244] and the Huguenots helped to supplant colportage in Vaud with regular trade carried on in booths and stores.[245] Aided by their connection with friends who had remained in France, they built up a thriving business in smuggling all sorts of cloth and other wares from France.[246] The French ambassador wrote Louis XIV, on May 31, 1687, that the refugees in Switzerland were trying to establish all kinds of shops and industries but that the market for their products was so small and the initial cost so great that few of them were succeeding. In this connection he especially mentioned a rug shop begun at Bern by two families formerly employed at Gobelins. On the other hand, he complained, many were engaged in smuggling goods through Geneva from Lyons and had perfected ways to avoid all customs officials so well that they told his wife all she had to do was to ask for a particular kind of French cloth or lace and that they would guarantee her delivery within a few weeks.[247] Still another Frenchman wrote from Geneva on June 20, 1708, that the Genevans—and especially the Huguenots there—were waxing wealthy as middlemen in the trade between France, Germany, and Italy and that some even had opened branch offices in Lyons to facilitate their illegal activities.[248] There is no doubt that a good portion of this sort of commerce received its impetus from the dislocations caused by war. Once the contacts were made and the channels cut, however, they persisted after the return of peace and help to account for the prosperity which developed during the second half of the eighteenth century.

## VII. CONCLUSIONS

Of the 200,000 Protestants who, I have estimated, fled France between 1680 and 1720, perhaps 40,000 or 50,000 settled in England, between 5,000 and 10,000 in Ireland, from 50,000 to 75,000 in the Dutch United Provinces, approximately 30,000 in German territory, and perhaps as many as 25,000 in Geneva and Switzerland. The rest migrated to other re-

[241] Archives Nationales, $G^7$ 1700 C (*registre*, pp. 206–7). Bertrand claims that the Huguenots "incontestably" introduced at Geneva the trades of making gloves, wigs, and lace, and perfected hatmaking and that branch of metalworking specializing in the construction of stocking-knitting looms (*op. cit.*, pp. 187–88).

[242] Gaberel, *op. cit.*, p. 110. See also Moerikofer, *op. cit.*, p. 240.

[243] Moerikofer, *op. cit.*, pp. 240–41.

[244] Guillebert, *op. cit.*, *BSHPF*, IV, 154.

[245] Combe, *op. cit.*, p. 151.

[246] *Ibid.*, p. 153; Bertrand, *op. cit.*, pp. 126–27.

[247] Archives du Ministère des Affaires Étrangères, *Ancien fonds*, "Suisse," *registre* 83, fols. 452–54.

[248] Archives Nationales, $G^7$ 1691.

gions in Europe, South Africa, and the New World. The governments of all the countries studied in this paper, with the exception of Switzerland, welcomed them and extended all kinds of financial aid and privileges in the hope that the refugees would be able to fortify their economies by introducing new industries and by imparting their skills and special techniques to the rest of the population. Most of the Huguenots hoped eventually to return to France and expected the allies fighting Louis XIV in the War of the League of Augsburg to insist upon an amnesty for them and a re-establishment of the Edict of Nantes as part of the terms of peace. When this did not occur, many of them still hoped that in one way or another the king or his successor could be persuaded to rescind the revocation of 1685. As a matter of fact, during a good part of the eighteenth century various schemes of persuasion and intrigue aimed at this goal were afoot. As long as the Huguenots thought of themselves as basically French, it is not surprising that they maintained their religious, social, and economic identity in no matter which adopted country they found themselves. This clannishness, together with their success in industry and trade and the special privileges and exemptions they enjoyed, helps to explain a great part of the popular displeasure over their presence. Save in some areas in Germany and Switzerland, the public's ill-will never became strong enough to repel the refugees or to nullify completely the effect they had as diffusers of technology. Since the rate of diffusion depends largely upon whether the channels of social communication are adequate and open, this resentment among the natives and this clannishness among the Huguenots must have interfered with the process of diffusion to some extent.

In 1685 England and Holland were not very far behind France industrially and commercially, if at all; whereas Ireland and Germany were. Switzerland, because of her limited resources and markets, lay somewhat in between. Hence the English and Dutch had less to learn from the French than did the others, but the very fact that their technological gap was not very wide made diffusion easier and quicker. The English silk industry acquired a new vitality after the immigrants introduced new patterns of cloth and new processes involved in making lustrous and figured fabrics. The sailcloth and fine linen branches, the manufacture of white paper, hatmaking, and many crafts producing items of luxury-quality increased in importance. In Holland the Huguenots developed various lines of the silk, velvet, linen, hat, paper, and glass industries and apparently participated in her shipping, fishing, and whaling activities. There is also some inferential evidence that they increased the supply of loanable funds. In Ireland they greatly improved the manufacture of linen and helped it to become the foremost industry of that economy. In Germany they introduced several kinds of textiles; they made ribbons, gloves, laces, woolen and silk stockings, and fine felt hats; they began the manufacture of blown and cast plate glass; they improved tanning and the hardware trades; and they brought additional land under cultivation and extended the margin of intensive farming. In Switzerland they engaged in a great number of personal service and other minor trades, contributed to the growth of cotton-weaving and cotton-printing in the eighteenth century, and tried—but without any great success—to develop the stocking, woolen, and silk industries. And, finally, they helped the little country to become a financial and commercial center.

If it were possible to draw up an account listing all the direct and indirect costs upon each country's economy and also the direct and indirect economic gains for which the Huguenots were conceivably responsible, I believe that the balance would be large in their favor. It is true that most of the costs were felt in the short run and that most of the gains were realized in the long run; but, even if the gains could be discounted over time, I believe that the account would more than justify on strictly economic grounds—not to mention social and moral considerations—the efforts made by the various countries to attract and retain them. The Huguenot migration did not promote in England an immediate and marked acceleration of economic growth; it did not prolong that remarkable period of prosperity in Holland which seems to have been coming to a close around 1680; it did not close the technological and economic gap separating Germany from France, Holland, or England; and it did not change Ireland and Switzerland into lands of plenty. During the last half of Louis XIV's reign, Europe generally evidenced less economic progress than either before or after this period, and some areas showed unmistakable signs of stagnation or even decline. Tariff wars, naval and land warfare, monetary disorders, and increased statism were forces too powerful to be completely counteracted by the 200,000 Protestants who disseminated their individualism, their economic drive, their skills and techniques, and their labor and capital resources abroad. Any historian would be rash indeed if he claimed that the revocation of the Edict of Nantes was the chief cause for the acceleration of economic development in western Europe after 1720–30. At the same time, however, he would be in error if he stated that the diffusion of technology resulting from the revocation was a wholly insignificant factor.

# THE EMPIRICAL STATUS OF BUSINESS-CYCLE THEORY

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Much criticism has been directed against business-cycle theorists for their failure to test hypotheses with available empirical data. In part, tests are not made because they always tend to be inadequate; but often even refutable hypotheses are not tested. As Friedman expresses it, "Because we cannot adequately test the validity of many hypotheses, we have fallen into the habit of not trying to test the validity of hypotheses even when we can do so. We examine evidence, reach a conclusion, set it forth, and rest content, neither asking ourselves what evidence might contradict our hypothesis nor seeking to find out whether it does."[1] The basic significance of such tests to the further development of business-cycle theory has also been emphasized by Tinbergen: "The business cycle is largely a measurable phenomenon, showing itself in the movement of many economic variables. It is a coherent movement but not rigorously cyclical. A large number of theories aim at explaining its nature. Most of those current in economic textbooks and literature run in qualitative terms. The need for testing these theories is now generally felt, because of their multiplicity, their mutual inconsistency, and their incompleteness or indeterminacy from the quantitative point of view. Testing will hence be of great significance, both for our factual knowledge of economic fluctuations and for the further development of the theories concerned."[2]

Before additional tests of business-cycle theories are made, however, it may be profitable to assess the current status of empirical research in business fluctuations. But it must be evident from the scope and breadth of this task that a thorough evaluation is not possible in a brief survey.

This paper brings together various approaches which have been made to the testing of hypotheses, along with results and critical comment on both results and methodology. The paper is developed in three sections. Section I summarizes certain past results of the testing of business cycles, along with some critical comment. Section II describes various projects currently under way and provides a brief evaluation of each. Finally, Section III suggests measures designed to increase the usefulness and significance of quantitative-historical research in business fluctuations.[3]

## I

### RESULTS OF ATTEMPTS TO GIVE EMPIRICAL CONTENT TO BUSINESS-CYCLE THEORIES

The general studies which have been made of empirical data over several busi-

[1] Milton Friedman, comment on paper of Carl Christ, "A Test of an Econometric Model for the United States, 1921-1947," in *Conference on Business Cycles* (New York: National Bureau of Economic Research, 1951), p. 107.

[2] Jan Tinbergen, "Reformulation of Current Business Cycle Theories as Refutable Hypotheses," *Conference on Business Cycles*, p. 131.

[3] One possible purpose for the paper must be disavowed. No attempt will be made to propose a new theory of business fluctuations or a general critique of business-fluctuation theories.

ness fluctuations, as a means of testing business-cycle hypotheses, employ three major approaches: the quantitative-historical, econometric, and National Bureau of Economic Research. In addition to general studies of business fluctuations, studies which are essentially historical and quantitative have been made of particular fluctuations, including those of 1865–79, 1919–21, and 1937–38. Attention will be directed first to the general studies of business fluctuations and then to studies of specific fluctuations.

### GENERAL STUDIES OF BUSINESS FLUCTUATIONS

*Econometric approach.*—One general approach to business-cycle research is that employed in econometrics. The task of this approach, according to Koopmans, is: "(1) to formulate all relevant hypotheses to which the available data may conceivably make an answer possible; (2) to extract from those data all information bearing on those hypotheses; (3) to select from the set of competing hypotheses the one hypothesis best supported, or the set of those hypotheses equally well supported, by the data; (4) to evaluate in some way the degree of confidence which can be placed in the rejection of the hypotheses not so selected."[4]

Major contributions with this approach have been made by Tinbergen,[5] Klein,[6] and Clark,[7] to name only a few. Tinbergen's study used forty variables and emphasized the special role played by stock prices in the 1929 depression. Klein added the impact of accumulated previous investment (the total capital stock) and treated the government more carefully. Clark built upon the Klein investment equation and paid special attention to inventory investment. In addition, he incorporated into his model Modigliani's discovery that the highest income previously earned influenced consumption outlay. Other models, of course, have been built and tested. But, so far, none of these models has been successful in predicting economic developments for periods not already included in the base for the predicting equations. The equations derived have fit the data very well, but this is not sufficient to demonstrate their usefulness. Thus, as Friedman comments, "the fact that the equations fit the data from which they are derived is a test primarily of the skill and patience of the analyst; it is not a test of the validity of the equations for any broader body of data."[8] Friedman has also suggested that the testing of these equation hypotheses is the crucial, but often neglected, step in validating empirical research.[9]

Recently Christ revised and subsequently tested Klein's model. This model, as had its predecessors, broke down when confronted with data outside the base period: "The predictions for 1948 made by the equations of the reduced form are, on the average over all equations, no better (measured by whether their errors are smaller in absolute value) than predictions made by naïve models which simply extrapolate either the value of each variable from the preceding year

[4] Tjalling Koopmans, "The Econometric Approach to Business Fluctuations," *Proceedings of the American Economic Association*, XXXIX (1949), 70.

[5] Tinbergen, *Business Cycles in the United States of America, 1919–1932* (Geneva: League of Nations, 1939).

[6] Lawrence Klein, *Economic Fluctuations in the United States* (New York: John Wiley & Sons, Inc., 1950).

[7] Colin Clark, "A System of Equations Explaining the United States Trade Cycle, 1921 to 1941," *Econometrica*, XVII (1949), 93–124.

[8] Friedman, *op. cit.*, p. 108.

[9] *Ibid.*, p. 107.

or the trend between the two preceding years."[10]

*National Bureau of Economic Research.*—A second general approach has been that of the National Bureau of Economic Research. This approach has been described by one writer as predicated on the assumption that "the cycle is a particularist phenomenon."[11] As Burns and Mitchell made clear in *Measuring Business Cycles*,[12] the Bureau approach consists in the identification of two types of average cycles, a reference cycle which depicts the behavior of a particular series during periods in which the economy as a whole is expanding or contracting and a specific cycle which presents the behavior of the series during periods in which the individual series is expanding and contracting. Criticism of the methodology of the National Bureau will be reserved for the next section. It is sufficient at this point to note a few of the tentative conclusions about business cycles presented by Burns and Mitchell: (1) Their study did not reveal significant long-run changes in amplitude or duration of a typical cycle;[13] (2) building cycles apparently do not affect the typical business cycle;[14] (3) there is some suggestion that the long wave of prices accentuates the duration of the contraction phase of the cycle;[15] and (4) there is substantial evidence that "business cycles vary rhythmically within the periods separating Juglar troughs."[16]

*Quantitative-historical.*—A third general approach has employed quantitative-historical analysis. Its characteristics have been described as follows: "The emphasis is on explaining behavior during particular cycles, rather than on obtaining general conclusions that apply to all cycles within a given period. Full use is made of qualitative as well as quantitative information. The approach does not require precise measurement of functional relations or the preliminary setting up of complete theoretical models. Causal inference depends upon personal interpretations and judgment after detailed examination of the available evidence. A variety of statistical techniques—including econometric studies—may be employed."[17]

The major studies employing these methods include those of Slichter,[18] Schumpeter,[19] Wilson,[20] and Frickey.[21] Slichter's study evaluated the significance of the period 1919–36 in business-cycle theory. He considered the questions suggested by the special emphases of the various theories of business cycles examined. In this way he was able to arrive at certain "tentative" conclusions

[10] Carl Christ, "A Test of an Econometric Model for the United States, 1921–1947," *Conference on Business Cycles*, p. 87.

[11] Edward Ames, "A Theoretical and Statistical Dilemma: The Contributions of Burns, Mitchell, and Frickey to Business-Cycle Theory," *Econometrica*, XVI (1948), 369.

[12] Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles* (New York: National Bureau of Economic Research, 1946).

[13] *Ibid.*, p. 464.

[14] *Ibid.*, p. 425.

[15] *Ibid.*, p. 438.

[16] *Ibid.*, p. 444.

[17] Robert A. Gordon, "Business Cycles in the Interwar Period: The 'Quantitative-Historical' Approach," *Proceedings of the American Economic Association*, XXXIX (1949), 54.

[18] Sumner H. Slichter, "The Period 1919–1936 in the United States: Its Significance for Business-Cycle Theory," *Review of Economics and Statistics*, XIX (1937), 1–19.

[19] Joseph A. Schumpeter, *Business Cycles*, Vol. II (New York: McGraw-Hill Book Co., 1939).

[20] Thomas Wilson, *Fluctuations in Income and Employment* (New York: Sir Isaac Pitman & Sons, Ltd., 1948).

[21] Edwin A. Frickey, *Economic Fluctuations in the United States* (Cambridge: Harvard University Press, 1942).

about the validity of business-cycle theories. It is evident, however, that Slichter was testing no particular, or at least consistent, model of business fluctuations.

Schumpeter's well-known three-cycle schema, because of its rigidity and mechanical aspects, tended to limit the scope of his examination of empirical data. In spite of this, his study was an outstanding contribution to the understanding of fluctuations. Nevertheless, there is reason to question the necessity which he seemed to feel for interpreting each ripple and wave of the many economic variables in light of the three-cycle schema. Of course, it is true that at various points he specifically denied that the validity of his theoretical model required the observed data to meet the timing criteria of the model. But, having said this, at other points he felt compelled to advance special reasons for the failure of any of the economic variables to perform in the way expected from his theoretical model.

A third study employing the quantitative-historical method was Wilson's, in which he examined the period 1919–39 in the United States. He tested some of the better-known hypotheses concerning business-cycle causation. His principal target was the monetary explanation which he found to be inconsistent generally with the economic data. He also derived a measure of support for the underconsumption hypothesis, particularly in the great depression.

A fourth quantitative-historical study was that of Frickey. He isolated a pattern of cyclical fluctuation and then studied secular-trend developments. This study, although illuminating the behavior of aggregative phenomena, failed to answer many of the questions raised by business-cycle theory. As one writer put it, "Frickey's ingenious and highly original approach is largely descriptive. He does not go far in answering questions in which theorists of business cycle policy are interested."[22]

Gordon has made three major points in criticizing the adequacy of the explanations provided for the interwar period by these quantitative-historical analyses: (1) Only Schumpeter considered the impact on the interwar period of secular movements, originating prior to 1914; (2) the quantitative significance of secular and cyclical developments in strategic areas is not discussed; (3) data are not analyzed in a "sufficiently detailed and systematic form."[23] Gordon's own approach to these problems is assessed below in the section on current research in business cycles.

### STUDIES OF SPECIFIC BUSINESS CYCLES

Finally, the various studies of particular fluctuations provide additional information on the empirical status of business-cycle theory. These include Fels's study of 1865–79,[24] which tended to support Schumpeter's theory that the turning point resulted because of a temporary saturation in investment outlets, particularly in railroad construction; Payne's study of 1919–22,[25] in which the turning point was adjudged the result of a buying strike on the part of consumers (a shift downward in the consumption

[22] Gottfried von Haberler, discussion of paper by R. A. Gordon, "Business Cycles in the Interwar Period: The 'Quantitative-Historical' Approach," *Proceedings of the American Economic Association*, XXXIX (1949), 85.

[23] Gordon, *op. cit.*, p. 55.

[24] Rendigs Fels, "American Business Cycles, 1865–79," *American Economic Review*, XLI (1951), 325–49.

[25] Wilson F. Payne, "Business Behavior, 1919–1922," *Studies in Business Administration*, XII (Chicago: *Journal of Business, Supplement*, XV [1942], 1–215).

function); and the several studies of the recession of 1937–38. Slichter,[26] Brockie,[27] Hayes[28] and Roose[29] all concluded that the recession of 1937–38 originated in investment factors rather than in consumption or oversaving.

This completes a brief statement on the various approaches which have been made to the problem of testing business-cycle theories with empirical data.

## II

### CURRENT EMPIRICAL RESEARCH ON BUSINESS-CYCLE THEORIES

Attention is now directed to current empirical research on business-cycle theories. It should be in order also to comment on the strength and weakness of each of the methods followed. Broadly speaking, empirical business-cycle research today, as in the past, is being carried on in three major directions: econometrics, National Bureau of Economic Research, and quantitative-historical studies. A cross fire of attack, criticism, and defense has been continuing between the model-builders whose approach is econometric and those who use the methodology of the National Bureau, with the quantitative-historical analysts at times attacking both.

#### ECONOMETRIC ANALYSIS

The difficulty with econometric analysis is, of course, that the enormous complexity of the real world resists reduction to a limited number of variables simple enough to be worked with and sufficiently reliable as an explanation of the causal factors that are operative. These points have been emphasized by Friedman in a statement in which he advises against further efforts in building models for the economy as a whole: "Does it not then follow that despite the unsatisfactory results to date, the appropriate procedure is to continue trying one after another of such systems until one that works is discovered? I think the answer is no. Granted that the final result will be capable of being expressed in the form of a system of simultaneous equations applying to the economy as a whole, it does not follow that the best way to get to that final result is by seeking to set such a system down now. . . . We now know so little about the dynamic mechanisms at work that there is enormous arbitrariness in any system set down. Limitations of resources—mental, computational, and statistical—enforce a model that, although complicated enough for our capacities, is yet enormously simple relative to the present state of understanding of the world we seek to explain. Until we can develop a simpler picture of the world, by an understanding of interrelations within sections of the economy, the construction of a model for the economy as a whole is bound to be almost a complete groping in the dark. The probability that such a process will yield a meaningful result seems to me almost negligible."[30]

The econometric approach has been attacked and defended from many quarters. One writer has described the work of the Cowles Commission as "petering out rapidly."[31] Even Koopmans concedes

[26] Sumner H. Slichter, "The Downturn of 1937," *Review of Economics and Statistics*, XX (1938), 97–110.

[27] Melvin A. Brockie, "The Rally, Crisis, and Depression, 1935–38" (unpublished doctoral dissertation, University of California, Los Angeles, 1948).

[28] Douglas A. Hayes, *Business Confidence and Business Activity: A Case Study of the Recession of 1937* (Ann Arbor: University of Michigan Press, 1951).

[29] Kenneth D. Roose, "The Recession of 1937–38," *Journal of Political Economy*, LVI (1948), 239–48.

[30] Friedman, *op. cit.*, pp. 112–13.

[31] Haberler, *op. cit.*, p. 84.

that the attempts to give more realism to the econometric model may be "defeated by less accurate estimation of any given parameter."[32] Still other writers have charged the econometric method with being too rigidly bound by the set of hypotheses chosen.[33] Econometricians themselves have met some of this criticism by acknowledging the basic and final importance of insight into the relationships of mathematics, economic theory, and the real world. Frisch's statement in this regard is quite informative: "No amount of mathematical technicality, however refined, can ever replace *intuition*, this inexplicable function which takes place in the brain of a great intellect who at the same time understands mathematics and economic theory in a more orthodox sense and who has lived long enough (or, more correctly, intensely enough) to accumulate human experience and a sense of facts. This intuition is an *art*, the art of realistic model building, the art of making realistic abstractions, the art in which such men as Ricardo and Schumpeter excelled. This intellectual activity is and will always be the vital part of our science and *the true criterion of an econometrician*."[34]

Econometricians have suggested various ways to meet some of these criticisms and to make econometrics into a more useful tool. Koopmans, for example, proposes "disaggregation" as the field for econometric study: "We can study investment in plant and equipment and investment in inventories, by industries or by industrial groupings. We can study consumption by important groupings of consumer goods. It is in these directions, among others, that further econometric work is waiting to be done."[35] Friedman's suggestions for future econometric research also point in the same direction: "The direction of work that seems to me to offer most hope for laying a foundation for a workable theory of change is the analysis of parts of the economy in the hope that we can find bits of order here and there and gradually combine these bits into a systematic picture of the whole. In the language of the model builders, I believe our chief hope is to study the sections covered by individual structural equations separately and independently of the rest of the economy."[36] If econometric research moves in this direction, then the gulf between it and the focus of both the National Bureau and quantitative-historical analysis should be narrowed.

### NATIONAL BUREAU ANALYSIS

The methodology and the direction of the work of the National Bureau of Economic Research have aroused a storm of controversy. Metzler, in an early review of *Measuring Business Cycles*, criticized the Bureau for its purely empirical definition of fluctuations which, in his opinion, can scarcely be refuted by any testing process. He further criticized the Bureau for lumping together cycles with greatly differing severity and content. He deplored, as well, the lack of adequate theory in the development of the model.[37] More recently, Gordon has charged that the approach establishes mechanical cri-

[32] Koopmans, *op. cit.*, p. 72.

[33] Cf., for example, Gordon, *op. cit.*, p. 52. See, however, the defense against this charge made by Tinbergen in "Reformulation of Current Business Cycle Theories . . . ," 135-41.

[34] Ragnar Frisch, "Some Personal Reminiscences on a Great Man," *Econometrica*, XIX (1951), 89-90.

[35] Koopmans, comment on paper by Jan Tinbergen, "Reformulation of Current Business Cycle Theories as Refutable Hypotheses," in *Conference on Business Cycles*, p. 145.

[36] Friedman, *op. cit.*, p. 114.

[37] Lloyd A. Metzler, review of Burns and Mitchell, *Measuring Business Cycles*, in *Social Research*, XIV (1947), 374-76.

teria for cycles which ignore the longer swings in economic life.[38] Wright has hurled a broadside against both the econometric and Bureau approaches because of their deterministic philosophies: "Personally, I happen to be a modified voluntarist. To me the past conditions the present but never wholly so. Always there is the elusive element of novelty and of self-determination. There cannot, I hold, ever be a perfect and complete science of history (in the sense of social forecasting). But whether one remains a philosophical determinist and holds that history is immutable—only that we have not yet learned its key—or whether one believes in an open system, it should be clear that the approach we have been sketching carries us far beyond the technique of either the Cowles Commission or the National Bureau. I simply do not believe that any set of econometric models, or any set of mathematical formulae, will ever suffice for reliable economic forecasting over any great length of time. The element of novel social conception is always breaking in."[39]

Somewhat more optimistic as to the staying power of the National Bureau approach has been Haberler. Thus, in reviewing the various approaches to study of business fluctuations, he notes that the National Bureau has been having "rather rough sailing recently. . . . Still it seems to have greater staying power than the other approaches mentioned, partly at the expense of extreme caution and unperturbable silence on many burning issues."[40] Regardless of the usefulness of the specific methodology of the National Bureau, it is generally agreed that the compilations and analyses of primary data which have come from the Bureau are invaluable and indispensable tools for a variety of approaches to the study of business fluctuations.

QUANTITATIVE-HISTORICAL ANALYSIS

The third form of current business-cycle research is quantitative-historical study. The proposals in Section III below, for increasing the usefulness of current empirical research in business cycles, come within the quantitative-historical approach. In this section, however, discussion will be confined to Gordon's interpretation of the method.

In his first paper[41] Gordon set forth his reasons for applying quantitative-historical analysis to the period 1919–40 and criticized some of the other approaches, as has been mentioned above. In this article he also raised a number of questions about the interwar period which he proposed to examine. Thus he asked, what was the nature of the secular forces? what was the role of World War I in influencing cyclical and secular developments? to what extent did investment opportunities "dry-up" at the end of the twenties? how may detailed studies of turning points be integrated with cyclical and secular factors? and some half-dozen other questions. Tentative conclusions concerning the nature of, and the causal factors operative in, the investment boom of the twenties have been presented in his second article.[42]

Like each of the other approaches examined, Gordon's approach has also

[38] Gordon, *op. cit.*, p. 54.

[39] David McCord Wright, comments on paper by Tinbergen, "Reformulation of Current Business Cycle Theories . . . ," in *Conference on Business Cycles*, p. 147.

[40] Haberler, *op. cit.*, p. 85. Recently this silence seems to have been broken. See, e.g., Moses Abramovitz, *Inventories and Business Cycles, with Special Reference to Manufacturers' Inventories* (New York: National Bureau of Economic Research, 1950).

[41] Gordon, *op. cit.*, pp. 47–63.

[42] Gordon, "Cyclical Experience in the Interwar Period: The Investment Boom of the 'Twenties,'" *Conference on Business Cycles*, pp. 163–215.

aroused criticism. The principal objection has been that he has failed to spell out or identify the theoretical framework underlying his analysis. As Angell puts it, Gordon does not present " 'explanatory' hypotheses, or any more general body of business-cycle theory, which might form the initial theoretical structure of his own work and be tested by it."[43] Burns suggests that Gordon "needs a more definite framework of analysis than he has presented."[44] Neisser, another critic, finds that the theoretical structure needs clearer expression.[45]

While encountering criticism in various quarters, at least Gordon's method is time tested. As Haberler suggests, Gordon's approach is "not new but well tried."[46] Juglar, Aftalion, Mitchell, Pigou, Robertson, and Schumpeter all employed quantitative-historical analysis in their study of business fluctuations. In fact, one of Schumpeter's last efforts was a plea for the historical approach to the analysis of business cycles: "What is really required is a large collection of industrial and locational monographs all drawn up according to the same plan and giving proper attention on the one hand to the incessant historical change in production and consumption functions and on the other hand to the quality and behavior of the leading personnel."[47] Support for the historical approach has also come from Kuznets: "If it could perform only one service—impress upon economists and other students of social problems the rapidity with which determining factors and their weights shift, and the variety of the forces that seem crucial in affecting the course of business cycles as they unfold over a sufficiently long stretch of historical experience in any given country—that service alone might be worth the effort. For it would prevent over-facile beliefs in the stability of multipliers and other aggregative ratios, and would intensify the demand that any generalization from historical experience be properly labeled with the time and space coordinates from which it was derived and to which it is supposed to apply."[48]

## III

### A MORE USEFUL APPROACH TO QUANTITATIVE-HISTORICAL BUSINESS-CYCLE RESEARCH

The suggestions in this final section are largely directed toward extending the usefulness of the quantitative-historical approach to business fluctuations.[49]

In the first place, it seems that the thinking and research of the last fifteen years or so have demonstrated the desirability of further business-cycle research which classifies and analyzes business fluctuations by concentrating on the components and determinants of the flow of national income. Thus, Haberler has raised the question: "Is it too much to say that there is also agreement that fluctuations in the aggregate flow of ex-

[43] James W. Angell, discussion of Gordon's "Business Cycles in the Interwar Period . . . ," in *Proceedings of the American Economic Association*, XXXIX (1949), 73.

[44] Arthur F. Burns, discussion of Gordon's "Business Cycles in the Interwar Period . . . ," in *Proceedings of the American Economic Association*, XXXIX (1949), 82.

[45] Hans Neisser, comment on paper by Gordon, "Cyclical Experience in the Interwar Period . . . ," in *Conference on Business Cycles*, p. 215.

[46] Haberler, *op. cit.*, p. 85.

[47] Schumpeter, "Historical Approach to the Analysis of Business Cycles," *Conference on Business Cycles*, p. 154.

[48] Simon Kuznets, comments on Schumpeter's "Historical Approach . . . ," in *Conference on Business Cycles*, p. 156.

[49] I am indebted to Professor Milton Friedman of the University of Chicago for critical comment on this section.

penditures is the *proximate* cause of fluctuations in output, employment, and prices?" And he has concluded: "It may be possible to find exceptions for particular industries, but for the fluctuations of business as a whole it seems to be fairly generally accepted."[50]

In the second place, it seems reasonable to believe that any sizable fluctuation in business activity must be the product of a number of factors. No change in any single variable seems adequate to initiate a recession or revival. In the recession of 1937–38, for example, the reduction in the government deficit gave a severe jolt to the economy but was insufficient by itself to reverse the rising national income. It took developments in other areas as well, such as cost increases, decreases in the profitability of investment, banking and other government policies, etc., to precipitate the recession.

In the third place, each of the comprehensive theories so far tested seems to have broken down when confronted with some particular fluctuation. This may not invalidate the theory as an explanation for a given class of fluctuations but raises serious doubts as to its applicability to all fluctuations: "We should realize that practically *all* theories of the business cycle could under some circumstances be correct, i.e., relevant. A slump, for example, can come because of too little purchasing power, but it can also come because of too much. Sometimes one force is the villain, sometimes another. And the logic by which forces succeed one another is never wholly ascertainable. The basic task not merely of cycle theory, therefore, but also of cycle forecasting, will be to compile *and* remember as large a catalogue of possible forces as may be, *then* to look at the 'facts,' mathematically described or otherwise. Personally I do not believe our catalogue can ever be complete. But at least if our approach seems distressingly tentative it will perhaps save us from the more disastrous possibilities that lurk in arrogant certainty."[51]

A fourth point which seems well worth considering is whether there actually is such a thing as a business cycle. There are business fluctuations, but are there business cycles? There are, of course, regular and periodic fluctuations among major economic variables, as, for example, in construction activity. It would also seem that some causal factors are operative in more than one fluctuation. Moreover, even if no business fluctuation were exactly like any other fluctuation, there may yet be *common fundamental* factors. There appears to be no reason to believe, however, that the movement of the economic variables makes each business fluctuation a replica of every other business fluctuation. A theory which attempts to explain *all* business fluctuations must take this into account, lest it result in abstractions which are far from adequate or realistic as explanations of developments in the real world. Perhaps too much time and effort have been given to determine the length of "business cycles" and their universal characteristics rather than to examine each fluctuation in terms of the observed economic variables and data.

This leads to a fifth observation. If business fluctuations are caused by a complex of factors, which are not necessarily cyclical in nature, then each business fluctuation has important elements of uniqueness and requires analysis in its own right. To the extent, therefore, that business fluctuations are unique, govern-

[50] Haberler, *op. cit.*, p. 87.

[51] Wright, *op. cit.*, p. 148. These same points have been made forcefully also by Haberler, *op. cit.*, p. 87.

ment intervention and appropriate policy become much more difficult. Nevertheless there remain certain very sensitive areas in all fluctuations where the crucial decisions altering the level of income and employment are made.

In summary, the approach suggested here for empirical research in business fluctuations would use the formal apparatus in which private-investment expenditures, consumption expenditures, and government expenditures uniquely determine the level of production, income, and employment. Such an approach is recommended in the belief that in this way progress may best be made toward the development of a theory which provides a more adequate explanation for business fluctuations. Moreover, this kind of quantitative-historical analysis will bring out the uniqueness of business fluctuations, while possibly at the same time confirming the belief expressed above that fluctuations in business bear similarities but are scarcely cyclical in form. The proposal is to provide a more definite and adequate framework for carrying on the general type of analysis engaged in by Gordon and his associates. It is not suggested that this type of analysis should be the exclusive approach to a study of business fluctuations or that it should be substituted for any particular approach now under way. But this approach provides "an emphasis on the unique and nonrecurrent features of any segment of historically bound experience."[52] And, in addition, careful examination of historical fluctuations should give more content to the varieties of theoretical explanations of business fluctuations.

[52] Kuznets, *op. cit.*, p. 161.

# SOCIAL CHOICE AND INDIVIDUAL VALUES[1]

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THE purpose of this article is to discuss the implications of Professor Arrow's conclusion that "if we exclude the possibility of interpersonal comparisons of utility, then the only methods of passing from individual tastes to social preferences which will be satisfactory and which will be defined for a wide range of sets of individual orderings are either imposed or dictatorial."[2]

Since Arrow's work may not be generally familiar, I will first give a brief summary of the central theorem.

## I. THE ARROW THEOREM

Imagine that there are $n$ possibilities ($n > 2$) and $m$ choosers ($m > 1$). Each chooser is deemed to have a self-consistent set of preferences ranging over all the possibilities and is assumed to choose or vote accordingly. Coalitions and bluffs are out of court; no one tries to be clever; everyone doggedly consults his own unswerving faith.

The problem is to find whether a self-consistent collective order of the possibilities, which obeys certain "conditions of correspondence" with the individual orders, is conceivable. So that the problem will be reasonably difficult, it is stipulated that there are at least three possibilities such that no conceivable set of individual orders thereof is ruled out a priori.

The "conditions of correspondence" laid down by Arrow can be formulated as follows:[3]

1. If one possibility rises or remains still in the order of every individual, then, *ceteris paribus*, it must not fall in the collective order.

2. If the removal from or insertion into the set of possibilities of a certain possibility $x$ results in no change in any individual order of the remaining possibilities, then it must cause no change in the collective order of those possibilities. This condition is named the "independence of irrelevant alternatives."

3. For all $x$ and $y$, $x$ must not rank above $y$ in the collective order, regardless of the individual orders. This is called the condition of "nonimposition."

4. The collective order must not coincide with the order of any one chooser, regardless of the orders of the other choosers. This is called the condition of "nondictatorship."

Arrow has proved that no consistent collective order satisfying these conditions is conceivable. In his terminology no satisfactory social welfare function is possible. By "satisfactory" is meant "satisfying the arbitrary conditions above." By "social welfare function" is meant "a process or rule which for each set of individual orderings for alternative social states (one ordering for each indi-

[1] A comment on a work by Professor Arrow. I am indebted to P. A. Samuelson, to G. Warnock, and to H. Brotman for various helpful comments on a draft of this article.

[2] K. J. Arrow, *Social Choice and Individual Values*, p. 59.

[3] Arrow actually has five conditions, but his first condition is not comparable with the others in that it merely sets the requirements for the problem indicated above, i.e., that the collective ordering be a true self-consistent ordering and that individual orders should not be unduly restricted a priori.

vidual) states a corresponding social ordering of alternative social states."[4] In this definition the possibilities are interpreted as the different possible states of society. A "social state" is defined thus: "The most precise definition of a social state would be a complete description of the amount of each type of commodity in the hands of each individual, the amount of labor to be supplied by each individual, the amount of each productive resource invested in each kind of productive activity, and the amounts of various types of collective activity."[5] The process of proof need not detain us here.[6]

## II. RELATION OF THE ARROW THEOREM TO "WELFARE" ECONOMICS

Let us suppose that each chooser orders different economic states strictly according to what he himself gets in each state. As is well known, this assumption enables one to isolate in principle an infinite number of "Pareto-optimum" states, each of which is defined by the impossibility of selecting any alternative state which is higher on one man's order and lower on no one's.

Bergson suggested that this comparatively useless theoretical result could be improved on only if an "economic welfare function" were postulated. What has this Bergson function to do with the Arrow function described above?

As I understand it, Bergson's welfare function was meant as a "process or rule" which would indicate the best economic state as a function of a changing environment (i.e., changing sets of possibilities defined by different economic transformation functions), *the individuals' tastes being given.* It may at once be noticed that Arrow's condition 1 above is inapplicable if tastes are given. Consequently, if I have interpreted Bergson's meaning correctly, then Arrow's result has no bearing on the possibility or impossibility of such a function.[7]

Arrow has a different interpretation. Thus he writes: "The ethical system is a rule which defines the Social state chosen from a given environment *as a function of the tastes of all individuals.* If, for a given set of tastes, the environment varies we expect that the choices will be consistent in the sense that the choice function is derivable from a weak social ordering of all social states. Thus the Bergson Social Welfare Function has the form of a rule assigning a social ordering to each possible set of individual orderings representing tastes."[8]

I should agree that we must expect that, in principle, for a given set of tastes, as the environment varies, the choices will be consistent in the sense that they are derivable from a weak ordering. In other words, all logically possible social or economic states (not merely all scientifically possible ones) must be ordered. Then, as the scientific possibilities change, different states are selected as best. This is my idea of a Bergson function; or, if it is not what Bergson meant, it is what I think he should have meant. This conception has two important points of difference from Arrow's conception. First, tastes are given. If tastes change, we may expect a new ordering of all the conceivable states; but we do not require that the difference between the new and the old ordering should bear any particular rela-

[4] Arrow, *op. cit.*, p. 23.

[5] *Ibid.*, p. 17.

[6] See *ibid.*, pp. 51–59.

[7] I am indebted both to the Rockefeller Foundation and to Samuelson for a conversation with the latter in which he pointed out this divergence between his concept of the Bergson function (as elaborated in his *Foundations of Economic Analysis*, chap. viii) and that of Arrow.

[8] *Op. cit.*, p. 72. Italics mine.

tion to the changes of taste which have occurred. We have, so to speak, a new world and a new order; and we do not demand correspondence between the change in the world and the change in the order.

The second important difference is that my interpretation of a Bergson function requires only that there should be an order. It does not require that it should be an order such that anyone would want to say of it that it represented the choices of society. To call an ordering a social ordering at all implies that one approves of the order, or of the mechanism (if any), through which it is determined by the individual choices or preferences. This is, of course, because "social" is a persuasive word. In discussing the logic of welfare economics there is no need thus to involve one's self in political philosophy. Thus the so-called "social welfare function," postulated by welfare economists, should on my view be regarded as a social ordering *only* in the sense that it orders states of society. Whether or not it is possible that such an ordering may be, in some further sense, a social ordering is a question we can, for the moment, leave on one side; we return to it later. The essential point here is that none of the advantages claimed for theoretical welfare economics, as a result of introducing such a function, depends in the least on the ordering of economic states being an ordering by society.[9] Instead of writing, with Bergson, $W = W(U_1, \ldots, U_n)$, we can write $W_i = W_i(U_1, \ldots, U_n)$ $(i = 1, \ldots, n)$. There is no need, so far as pure theory is concerned, to introduce a further (social) welfare function of the form $W = W(W_1, \ldots, W_n)$. We can deduce the whole effective corpus of welfare economics from, say, $W_{10} = W_{10}(U_1, \ldots, U_n)$—remembering only to put "in the opinion of individual No. 10" after "welfare" whenever we use the term.

Arrow wants to go much farther than this in two directions. He wants a "process or rule" which will produce a social ordering as a function of the tastes themselves. As I have tried to indicate, neither of these requirements really has anything to do with what is commonly thought of as welfare economics. Traditionally, tastes are given; indeed, one might almost say that the given individuals are traditionally defined as the possessors of the given tastes and that no sense is attached to the notion of given individuals with changing tastes (certainly the individual's "economic welfare" is not supposed to be comparable before and after a change of taste).

Quite apart from condition 1, Arrow's other conditions are defined in terms of changing tastes. Nevertheless, it is condition 1 which is crucial, because the other conditions have analogues if tastes are given. Condition 2 need merely be modified to read, "The removal from or insertion into the set of possibilities of a certain possibility $x$ must cause no change in the collective order of the remaining possibilities." For, if the entry or exodus of a possibility *did* cause a change in the individual orders of the remainder, we should be faced with a new set of tastes which would require a new "master"-order,[10] which, on my view, need not bear any relation to the old order. Condition 3, called "nonimposition," can be modified to state simply that there must be some conceivable set of individual orders which precludes any given "master"-

[9] It is, incidentally, clear from Samuelson's work (*op. cit.*) that he would agree with me about this.

[10] I shall, where necessary, refer to a "master"-order throughout the rest of this article, in order to try to avoid begging questions about the nature of the order.

order. In this form, "nonimposition" is a traditional assumption of welfare economics equivalent to the postulate that if someone prefers $x$ to $y$ and no one $y$ to $x$, then $x$ is better than $y$. Condition 4 ("nondictatorship") must also, if tastes are given, be modified to the effect that the "master"-order must not coincide with that of any one individual if this order is exactly reversed for all others.[11]

While we must conclude that Arrow's work has no relevance to the traditional theory of welfare economics, which culminates in the Bergson-Samuelson formulation, it remains, of course, true that his wider conception may have advantages of its own. Let us therefore examine his conditions on their own merits.

First, consider condition 1. Suppose that we have three individuals (defined independently of their tastes) and that the orders of three possible states are $xyz$ for Mr. A, $zxy$ for Mr. B, and $yzx$ for Mr. C. Let the "master"-order be $zxy$. Then, let there be a change of taste such that $z$ rises or remains still in each order, letting the orders become $xzy$ for Mr. A, $zxy$ for Mr. B, and $yzx$ for Mr. C. Condition 1 requires that $z$ remain top of the poll. But, looking at the matter from an Olympian point of view, one can ask, "Why on earth should it?" Could not, for instance, A's change of taste as between $y$ and $z$ justify a higher weighting of his first choice, so that $xyz$ should become the "master"-order? There is, indeed, nothing in Arrow's theory which prevents us from supposing that the rise of $z$ in the first order is due not to a change of taste on the part of an ostensibly defined Mr. A but rather to the substitution of a new individual for Mr. A. There is then even less reason why one should accept the view that $z$ must remain top of the poll. I may want to attach a high weight to the new man's tastes.

Take now condition 2, called the "independence of irrelevant alternatives." Arrow himself gives an example which throws some doubt on this condition. He writes: "Suppose that there are just two commodities, bread and wine. A distribution, deemed equitable by all, is arranged, with the wine-lovers getting more wine and less bread than the abstainers. Suppose now that all the wine is destroyed. Are the wine-lovers entitled, because of that fact, to more than an equal share of bread? The answer is, of course, a value judgement. My own feeling is that tastes for unattainable alternatives should have nothing to do with the decision among the attainable ones; desires in conflict with reality are not entitled to consideration, so that condition (3) . . . is a valid value judgement, to me at least."[12] It is, however, easy enough to argue the other way. Thus I can argue that if sacrifices must be made in an equalitarian society, then they should be equal sacrifices. But if the wine-lovers got only an equal share of bread, they would lose more heavily than the bread-lovers. What one would think in an actual case would depend so much on so many circumstances. At all events, I know that I should hate to commit myself a priori to Arrow's value judgment.

Condition 3 (nonimposition) needs no comment, since it has been discussed *ad nauseam* in the literature.

As put by Arrow, condition 4 ("nondictatorship") appears eminently acceptable. In his formulation, Tom is a dictator if, for all $x$ and $y$ and "regardless" of the orderings of others, "Tom prefers $x$ to $y$" implies "$x$ is higher in the master-order." What Arrow evidently

[11] This form of the "nondictatorship" condition is, in any case, a consequence of the above conditions. See Arrow, *op. cit.*, pp. 53–54.

[12] *Op. cit.*, p. 73.

has in mind is that if no change in the orders of others can upset the coincidence of Tom's and the master-order, then it would seem reasonable to call Tom a dictator. Here I should agree. But, when we discover that this condition, if combined with the others, produces the consequence that no more need be known to dub a man "dictator" than that his ordering of any pair of alternatives has prevailed against the reverse ordering on the part of everyone else, we may well begin to feel uncomfortable.

Take an example. Let there be three men and two alternatives, $x$ and $y$. Let the orders be $xy$ for Tom and $yx$ for both Dick and Harry. The conditions then preclude $xy$ as the master-order. The two (economic) states may be such that in $y$ Tom has one piece of manna, while Dick and Harry both have ninety-nine pieces; in $x$ Tom has three pieces and Dick and Harry both have ninety-eight pieces. Might not then the master-ranking $xy$ be desirable? Use of the word "dictatorship" suggests that Tom's preference for $x$ over $y$ somehow causes society to choose $x$. But, in fact, the coincidence of Tom's and the master-order may be a result of his poverty and not of his power. The plausibility of the "nondictatorship" condition may come partly from the ambiguity of the word "imply." In ordinary language it carries a suggestion of causation. But in logic "$p$ implies $q$" means no more than "not both $p$ and not $q$." Thus poor Tom is deemed dictatorial merely because it happens to be the case that society agrees with him and not with the plutocrats. This may be an absurdly limited example (there being only three men and two alternatives), but, presumably, Arrow intended his conditions to be perfectly general. He does, it is true, find this sort of consequence paradoxical;[13] but he draws no conclusion from the paradox. The conclusion, to my mind, is that it is foolish to accept or reject a set of ethical axioms one at a time. One must know the consequences before one can say whether one finds the set acceptable—which fact sets a limit to the usefulness of deductive techniques in ethics or in welfare economics.

It is evident that Arrow has introduced a most interesting logical framework for possible critiques of the behavior of economic systems under conditions of changing tastes and changing income distribution. He fills out the framework with a set of value judgments which yield him entirely negative conclusions. The fact that I have argued that I can see no reason to accept, and some reasons not to accept, his conditions does not, of course, imply that I think that any generally acceptable set of value judgments, which would turn the logical framework into something useful, is likely or even possible. In fact, I think the opposite, as should become clear from what follows.

## III. THE GENERAL VERSION OF THE ARROW SOCIAL WELFARE FUNCTION IN ITS RELATION TO ETHICS AND POLITICAL PHILOSOPHY

In the preceding section it was supposed that each individual ordered states solely according to what he himself would get. Everything else which individuals thought significant could play its part only in determining the form of the function which would produce the "master"-ordering from these original orderings. But of course there is no need to suppose this. In general, Arrow assumes that the individuals take more into account than their own utility when compiling their original orders; more, indeed, than other people's utilities or welfare. In fact, quite generally, we may suppose that they arrange all states in order of what they regard as ultimate desirabil-

[13] E.g., *ibid.*, p. 55.

ity, taking everything they know and feel to be significant into account. We shall call such an ordering a value ordering, although, of course, many of the discriminations made would not really be felt sufficiently strongly to be called value judgments. The problem, then, is to form a "master"-ordering; in Rousseau's language the problem is the well-known one of discovering the general will.

We must now make an important distinction which Arrow fails to draw. He calls his function both a social welfare function and a decision-making process. He believes that "one of the great advantages of abstract postulational methods is the fact that the same system may be given different interpretations permitting a considerable saving of time." Yes, but we must be careful not to give such a system a nonsensical interpretation, and it will be my contention that to interpret it as a social welfare function *is* to give a nonsensical interpretation.

Imagine the system as a machine which produces a card on which is written "$x$ is better than $y$," or vice versa, when all individual answers to the question "Is $x$ better than $y$?" have been fed into it. What significance are we to attach to the sentence on the card, i.e., to the resulting "master"-order? First, it is clear that the sentence, although it is a sentence employing ethical terms, is not a value *judgment*. Every value judgment must be *someone's* judgment of values. If there are $n$ people filling in cards to be fed into the machine, then we have $n$ value judgments, not $n + 1$. The sentence which the machine produces expresses a ruling, or decision, which is different in kind from what is expressed by the sentences fed into it. The latter express value judgments; the former expresses a ruling between these judgments. Thus we can legitimately call the machine, or function, a decision-making process.

But what would it mean to call the machine a social welfare function? One would be asserting, in effect, that if the machine decided in favor of $x$ rather than in favor of $y$, then $x$ would produce more social welfare than $y$ or simply be more desirable than $y$. This is clearly a value judgment, but it is, of course, a value judgment made by the person who calls the machine a social welfare function. Thus, in general, to call the machine a social welfare function is to assert that $x$ is better than $y$ whenever the machine writes the sentence "$x$ is better than $y$." Now we may suppose that the individual who calls the machine a social welfare function is one of those who has fed his own value order into it. It is clear that this person must be contradicting himself unless the "master"-order coincides with his own value ordering. It follows that if the machine is to be called a social welfare function, then anyone who is called upon to accept or reject the principles on which it is built (i.e., the conditions of correspondence) must refuse to accept any principle which insures that the "master"-order will necessarily not coincide with his own. This is because the conditions of correspondence determine the "master"-order and because by calling the machine a social welfare function the person in question has accepted the "master"-order. (It should be noted that accepting or rejecting a value sentence entails agreement or disagreement with the corresponding value judgment.) In other words, it is inconsistent both to call the machine a social welfare function and to accept the condition of nondictatorship.

This may appear very surprising. First, it may be objected that, as in the case considered in the previous section, a rich man may, like Bernard Shaw, easily *prefer* a state which yields him more manna but at the same time ***disapprove*** of it because he is an equalitarian and would therefore accept a rule which named as best a social state which yielded him less manna. This objection does not, however, apply in the case under consideration, because in this most general version it is presumed that the individual orders take the welfare of others into consideration. He would therefore *not* rank as higher the state which yielded him more manna.

It is true, of course, that, in the initial ordering, one thing which a person cannot take into consideration is other people's valuations, which he may very well regard as highly significant, if he is a tolerant sort of person. Even worse is the fact that valuations are expressed in value statements, whose whole purpose is to influence other people's value judgments and behavior. It would consequently be surprising if an individual were entirely uninfluenced by the verdicts of others. Indeed, when a working agreement is reached, it is usually partly, if not wholly, the result of this kind of mutual influence. These are difficulties which Arrow does not seem to face. Nevertheless, they can, formally at least, be side-stepped. Thus we can suppose that we can have as much revoting, or rearrangement of individual orders, as the individuals want when they learn the opinions and verdicts of each other. Accordingly each order is, so to speak, a deadlock order. It is the individual order resulting when all persuasion has been used, and after an indefinite number of straw votes.[14]

Second, it will be objected that I may regard $x$ as better than $y$ but still be willing to submit to a ruling which proclaims $y$ as society's choice. But it can easily be seen that this, though true, is no objection, for it interprets the machine as a decision-making process. I am not maintaining that there is anything inconsistent about accepting the nondictatorship condition when the machine is *thus* interpreted. The acceptability of the conditions of correspondence, given the decision-making-process interpretation, will be discussed in the next section.

Third, some people might not find convincing my statement that the sentences which the machine produces cannot themselves be regarded as value judgments. They may think that judgments do not necessarily require someone to judge. Though I believe it to be a nonsensical supposition, let us suppose, for the remainder of this section, that the machine *does* produce value judgments.

Now if I think $x$ is better than $y$, and the machine announces that $y$ is better than $x$, I have three choices. First I can say: "I was wrong; $y$ *is* better than $x$." Thus Rousseau claims: "When therefore the opinion that is contrary to my own prevails, this proves neither more nor less than that I was mistaken."[15] Rousseau's remark can be taken to mean merely that I was wrong in that my original vote was a guess at the general will which, *ex hypothesi*, is the majority decision. But Rousseau is nothing if not inconsistent, and the remark could be taken as an appeal to some mystical standard of right and wrong. But since few philosophers now believe that value judgments can be verified or falsified (i.e., be said to be true or false in any objective sense), and

[14] This consideration alone would seem to limit the applicability of the Arrow theorem to a very small society. Moreover, it is quite reasonable to suppose that no such deadlock, or equilibrium, set of orders would ever result, especially in a large society. The dynamics of value formation may imply that values are, as a result of mutual influence, in a state of permanent flux.

[15] *Social Contract* ("Everyman" ed.), p. 94.

since the fact that they cannot be thus verified is the whole basis of Arrow's work, this choice can be dismissed. Second, I can say: "I have changed my mind. I now wish to assert the same value judgment as the machine; *y is* better than *x*." It is quite clear that we can dismiss this as a possibility for everyone; for if everyone always changed his values to be in conformity with the machine, no problem could arise. There would always be unanimous agreement in the end, however perverse the machine was. Third, I can say "To hell with the machine; *x* is better than *y*, whatever it says." If I always did this (and how could I do anything else, since, remember, we are supposing that all other people's values are already known; indeed I have taken into account everything I think significant!), then I should naturally have refused to accept the condition of nondictatorship. It is in the nature of value judgments that the only order which I can fully accept is one that coincides with my own, regardless of the orders of other people. In other words, no one can consistently accept the condition of "nondictatorship." At all events, it is sheer nonsense to say, "A is better than B, but everyone else claims B is better than A; therefore, since I don't want to be a dictator, B is better than A." Yet this is what acceptance of condition 4 would imply in this general case. Once again we are back at the conclusion that condition 4 makes nonsense of any interpretation of the Arrow system in ethical terms. In my opinion, this makes the title "social welfare function" a dangerous misnomer.

Arrow presumably thinks differently, though it is difficult to be sure of this from his book. Thus, having shown that a "social welfare function" satisfying his conditions is possible if individual orders happen to be of a certain form,[16] he proceeds to a brief discussion of some of the politicoethical systems which various philosophers have vainly proposed and rightly says that they are based on some idea of an initial consensus of values; he then writes: "The results . . . show that the condition of unanimity is mathematically unnecessary to the existence of a social welfare function, and we may well hope that there are still other conditions [i.e., conditions as to the form of the individual valuations] . . . under which the formation of a social welfare function will be possible."[17]

The very use of the phrase "social welfare function" suggests that if such were possible, then somehow an objective moral code would have been erected. Political philosophers used to worry over the question "Why ought I to obey the state (or society, or the general will)?" The pseudo-puzzle of how one can both be free and be subject to law is a variant of the pseudo-puzzle of how duty and self-interest can be reconciled. The philosophers tried to show that it was always really in one's own interest to abide by whatever the "objective" code might be. Since, plainly, conflict must arise, at least in the absence of complete initial consensus, and since such consensus was obviously absent, they invented the doctrine of a metaphysical consensus. When people actually (i.e., really) disagreed about some matter affecting the common interest, they were really (i.e., metaphysically) agreeing. Rousseau, who was first responsible for this nonsense, found some difficulty in explaining how one found out what people were really metaphysically agreeing about when they disagreed; i.e., he found some difficulty in

[16] So-called "single-peaked" preferences. See Arrow, *op. cit.*, chap. vii, Par. II.

[17] *Ibid.*, p. 83.

explaining the circumstances under which one could be sure that the general will had expressed itself. We may illustrate all this with a few quotations: "The problem is to find a form of association which will defend and protect with the whole common force the person and goods of each associate, and in which each, while uniting himself with all, may still obey himself alone, and remain as free as before."[18] "Each of us puts his person and all his power in common under the supreme direction of the general will."[19] "Whoever refuses to obey the general will shall be compelled to do so by the whole body. This means nothing less than that he will be forced to be free."[20] "The general will is always right and tends to the public advantage."[21]

It is but a very short step from here (a step which Hegel took) to maintain that acceptance of the social order (or obedience to the state) is really only self-obedience. Arrow's problem clearly has so much in common with Rousseau's that it seems worth while to point out again the insidious danger of this approach. Modern totalitarian philosophy may be not altogether unjustly fathered on Rousseau.

Ever since the objectivity of utilitarianism was called in question, economists have been searching for that old philosophers' stone, a scientific ethic. Recently many philosophers, those of the linguistic school, have given up the search, because they have realized that any attempt to define ethical terms is necessarily also an attempt to lay down a moral code; i.e., any supposed definition of an ethical word in nonethical terms is itself an ethical judgment. Thus, in view of the undoubted fact of disagreement about values, any assertion that such-and-such a given value statement is "objectively true" is itself a value statement which persuasively twists words like "objective" and "true" in an attempt to get the given value statement accepted.

We can have an "objective" ethic only when there is universal agreement. But when there is universal agreement ethics will have ceased to exist, since all moral judgments will have become futile and redundant. One does not tell people that they ought to do what they are doing or will do anyway. If everyone agreed, there would be no need to try to change other people's values and behavior, which is the primary use of value statements. In Utopia, not only will the state have withered away, but also all morality; and naturally, since it is Utopia, not only all morality, but also welfare economics.

## IV. THE GENERAL VERSION OF THE ARROW SOCIAL WELFARE FUNCTION IN RELATION TO POLITICS AND DECISION-MAKING

We indicated in Section III that the so-called "social welfare function" must be interpreted not (as the name might seem to imply) as a social welfare function but as a decision-making process. The question arises whether one can consistently accept Arrow's conditions, given this interpretation.

The importance of the distinction between a value order and a decision order, for our purposes, is as follows: In a given community, or committee, as many value orderings as there are individuals may coexist. On the other hand, as between two alternatives there can be only one effective decision. Thus we may all have our conflicting opinions as to whether we ought to go to war or not.

18 Rousseau, *op. cit.*, p. 14.

19 *Ibid.*, p. 15.

20 *Ibid.*, p. 18.

21 *Ibid.*, p. 25.

But the decision to do so or not to do so is unique and binding for everyone. Where values are concerned, everyone must be a "dictator" (i.e., the logic of value judgments is such that one cannot consistently *accept* any value ordering which differs from one's own); where decisions are in question, everyone cannot be a dictator, in Arrow's sense, unless there is unanimity.

Thus an individual will often be prepared to accept a decision which goes against him, because the same decision-making process (or "procedure," for short) will be used for making many other decisions between other alternatives, some of which will go in his favor. This is, of course, true; but it is not an answer to the problem of why acceptance of Arrow's conditions does not imply inconsistency. I may be prepared, of course, to accept decisions which go against me because it is unwise to rebel against an established procedure with established sanctions. But this does not explain why, when I am free and still without my chains, I should accept certain a priori conditions to which any social contract must conform—a priori conditions which rule out all procedures which would ever decide in my favor against everyone else. To explain this, we require, I think, the hypothesis that, among the very limited number of procedures which would stand any chance of being established, none would make me dictatorial in Arrow's sense, together with the hypothesis that all such procedures would determine social states higher on my valuation than anarchy.

It may be suggested that the matter is much simpler than this and that I am making possible acceptance of the conditions depend too much on expediency and not enough on high moral purpose. Thus it may be said that, although one values $x$ above $y$, one might prefer to accept a procedure which would yield $y$, if the only way to get $x$ were to impose one's will on society.[22] This is true; but it is not a sufficient reason for accepting Arrow's condition of "nondictatorship," which, barring the name, contains nothing about power or force. This is not, of course, Arrow's fault, given his technique. It springs from the fact that causal and power relations are not logical relations. Thus, since "dictatorship," in the ordinary sense, involves the idea of causation, it cannot be adequately described in terms of implication. Even so, one must add that it is probably true that most people would attach intrinsic demerit to a procedure which ever allowed a minority of one to win.

To sum this up: One may say that expediency might, in most situations, counsel one to accept condition 4 as applied to procedure. It may also be widely acceptable on moral grounds. Indeed, conditions 3 and 4 might be said to contain the bare essentials of democratic procedures—but, perhaps, only the barest essentials, since there is nothing in them

[22] It should be noted that to formulate this suggestion at all it is necessary to assume that the "social states" contain only descriptions of the goods accruing to each individual. It presupposes that the procedures have intrinsic, and not merely derived, merit or demerit. In other words, since we are now dealing with decision-making processes or mechanisms, means may be valued apart from the ends attained by them. This distinction of "ends" and "means" could not, of course, arise when we were considering (or, rather, trying to consider) the function as a social welfare function. There cannot be procedures or mechanisms (means) for working out what is right or wrong. Means are, in the present context, means of arriving at a decision. It should be noted, however, that "means" usually refers to social states which would necessarily precede some more distant social state aimed at. In this sense, the means are part of the ends in the present discussion: the individuals in willing the ends necessarily also will the means; they order the alternative future chains of social status.

to prevent, say, a minority of two in a million from carrying the day.

We may now briefly consider conditions 1 and 2, taken in conjunction with the others. It would seem unlikely that anyone would want to attach intrinsic merit or demerit to them. Consequently, one may suppose that individuals would decide whether or not to accept them on the basis of whether, in conjunction with the remaining conditions, they would be likely to admit or exclude procedures more or less likely to lead to higher or lower states more or less often than alternative procedures. Thus the value of such conditions of correspondence would be derivative, though not derivative solely from one's own present, or even expected future, ordering, but also from a consideration of probable changes in other orderings as well (e.g., if I disapproved of drunkenness very much and thought the taste for gin on the increase, I might not wish condition 1 to be satisfied for states involving alcohol). The chief difficulty with any such "conditions of correspondence" is that, for both logical and factual reasons, it is exceedingly difficult to see what they involve one in. Nevertheless, Arrow's conditions undoubtedly have some prima facie plausibility, probably mainly for the reasons of expediency discussed above.

## V. CONCLUSIONS

It was first concluded that Arrow's system is quite different from, and has little relevance to, traditional welfare economics. Second, it was found that it cannot without inconsistency be interpreted as a critique of social welfare functions. Last, we found that it, and no doubt similar deductive techniques, are in principle applicable to decision-making processes. Whether, in this sphere, Arrow's conditions of correspondence are sufficiently acceptable as minimum conditions for a satisfactory democratic decision-making procedure for it to be said that he has proved that consistent decisions cannot be reached via such procedures must be left an open question. Some people will no doubt think that such a proposition requires no proof. Others will think that the essence of democracy is something which must escape definition in terms of any functional relation between decisions and individual preferences.

# TEN GREAT ECONOMISTS[1]

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THE ten great economists treated by Professor Schumpeter in this volume are Marx, Walras, Carl Menger, Marshall, Pareto, Bohm-Bawerk, Taussig, Fisher, Mitchell, and J. M. Keynes; in addition, short notices on Knapp, Wieser, and Bortkiewicz are included in an appendix. All of the ten essays have been published previously, the first, on Marx, constituting Part I of *Capitalism, Socialism, and Democracy*, the others being journal articles which appeared either upon the death of the particular economist or upon some anniversary. Schumpeter himself selected these essays, with the exception of the first, for a collection of the present character. Also, with the exception of Marx, all the subjects were persons whom Schumpeter knew, sometimes very well, though the close personal relationships did not include Keynes (see the excellent Foreword, written by Elizabeth Boody Schumpeter).

Just why Wieser, for example, whom Schumpeter judged to be the "intellectual peer" of Menger (p. 89), was nevertheless not included among the great is not easily understood. But the title is simply "ten great"; and Schumpeter says, "I do not believe in grading economists." He departs from this self-limiting ordinance, however, far enough to declare Walras to be the "greatest of all theorists" (p. 95).

In general, the essays assume conversancy with the authors and their works; they are addressed to Schumpeter's professional colleagues and depend for their richness of meaning and implication upon an audience well seasoned in economic analysis, in the history of economic thought, and in general cultural history. For such readers the sparse biographical material will help to clarify Schumpeter's interpretation of his subject; and they will welcome the seriatim mention and ordering of the chief works. But these are not encyclopedia articles, and they would not well serve the purpose of conveying for the first time, even to a very able graduate student in economics, the substance of the theories being discussed. They stand, as they originally appeared, as appraisals of the life and work of figures already quite familiar to a knowledgeable audience.

The paper on Marx takes up one-fourth of the book. Except for some not too clearly specified insights into economic fluctuations, one after another of the famous Marxian dogmas are weighed and found wanting. Why, then, this pride of place and this amount of space devoted to Karl Marx? Because "his result might nevertheless be true so far as it simply avers that capitalism will destroy the foundations of capitalist society. I believe it is" (p. 53). Schumpeter feels a great affinity *in general* with another (one might almost say "the other") who has wedded an economic analysis with a philosophy of history. But *in particular*, the affinity rests upon the supposed self-destruction of capitalism. Schumpeter's criticism of such Marxian treasures as the increasing misery of the proletariat, the class war, the materialistic interpretation of history, and the labor theory of value are all the more telling by reason of their emanating from an admiring critic. The only mystery—after this display of perception and prowess—is the admiration.

Next follow two brief notices on Walras and Menger and a longer appreciation of

[1] A review note of *Ten Great Economists from Marx to Keynes*. By Joseph A. Schumpeter. New York: Oxford University Press, 1951. Pp. xiv+305. $4.75.

Alfred Marshall written on the fiftieth anniversary of the *Principles* in 1940. Schumpeter quite properly accords praise to Marshall, first for his habit of letting economic theory emerge from a rich context of historical fact and, second, for his essentially mathematical methods of thought. Besides supplying the intellectual leadership for a whole generation of English economists, Marshall provided paths toward future developments in period analysis, in imperfect competition theory, in aggregative analysis (in Book VI), in the theory of trends or economic evolution, and, finally, through the formulation of precise quantifiable concepts (such as the Marshallian demand-and-cost functions) in econometrics.

We pass from the founder of the Cambridge school of economics to another founder of a genuine school (*rara avis*), Vilfredo Pareto, who succeeded where Walras had failed in forming an "inner circle" at Lausanne. Schumpeter defends Pareto against those who would impute Fascist leanings to him; he was, indeed, a rigorous liberal but *anti-étatiste*, who "fought with impartial ferocity the [Italian] governments that succeeded one another" (p. 116). As for his economic theory, Schumpeter lays great emphasis upon Pareto's indifference function technique, and upon this basis concludes that "we must indeed salute either Fisher or Pareto as the patron saint of the modern theory of value" (p. 130). I humbly beg leave to differ: indifference functions are only a competing graphical device, more elegant for exhibiting income as the independent variable but less elegant than Marshallian utility and demand functions for exhibiting price effects. As Robertson has recently concluded, diminishing marginal utility for the individual person is a prerequisite of welfare economics,[2] and, indeed, I would add, of *value* theory. But Schumpeter does not err in emphasizing Pareto's great role in the development of welfare economics and of the theory of production.

The essay on Böhm-Bawerk seems unnecessarily long, even with a reduction in length by one-half at the hands of the translator, Professor Herbert Zassenhaus, and of Professor Haberler. The enormous enthusiasm which it reveals for Böhm's theory of interest is difficult to account for in this day and age, except perhaps in terms of loyalty and the date of publication (1914). Schumpeter's support of Fisher's theory of interest simply does not house logically with most of the Böhm-Bawerkian structure. Time preference and productivity figure of course in both. But Böhm carries along a heavy loading of impedimenta from the classical wages-fund doctrine and the labor theory of value. Land and labor as "original" factors of production, wages and rent as "discounted marginal product," the subsistence fund advanced by capitalists, the fund as determinative of the length of the "period of production," the view of capital as essentially working capital on its way toward consumption—all these find no place, and quite properly so, in current theories of interest. Only if no Menger, Walras, or Jevons had intervened since the day of Ricardo would Böhm's *Positive Theory* have been noteworthy.

Unlike the other essays, the one devoted to Taussig is largely biographical. It deals sympathetically and justly with Taussig's long and distinguished career as teacher, editor, university officer, public servant, and practical analyst of such issues as silver and, more notably, the American tariff. But the essay, almost inevitably as the product of a co-operative effort with two other Harvard professors, lacks the distinctive flavor of Schumpeter; and it does not go far toward fitting the subject into the evolution of economic ideas.

The chief motif of Schumpeter's appraisal of Irving Fisher is his "unification of the theoretical-quantitative and the empirical-quantitative approach," which runs through all Fisher's major works from the *Mathematical Investigations in the Theory of Value and Prices* (1892), *Appreciation and Interest* (1896), *The Nature of Capital and Income*

[2] D. H. Robertson, "A Revolutionist's Handbook," *Quarterly Journal of Economics*, February, 1950, pp. 6–7.

(1906), *The Rate of Interest* (1907), *The Purchasing Power of Money* (1911), to *Booms and Depressions* (1932). Throughout this long period Fisher consistently strove to formulate his theories in such wise as to permit statistical verification or criticism. This effort earns for him Schumpeter's praise as "the most important of the pioneers of econometrics since William Petty," and the fruits of his labors evoke Schumpeter's prediction that Fisher's name "will one day stand as the name of this country's greatest scientific economist" (p. 223), another departure from his own rule against grading economists. But the departure is warrantable, because it is not far wide of the mark and because Schumpeter, of all the really distinguished students of the history of economics, is best qualified—by reason of an objectivity given by his European extraction and culture, as well as a familiarity assured by his many years in America—to express such a judgment.

Schumpeter regrets that Fisher did not assemble the wealth of economic theory embodied in his six great books into a systematic magnum opus; and with this all admirers of Fisher's penetrating intellect must agree. Had he done so, it seems probable that he would have founded a school of thought equal in dignity and influence to the Marshallian school at Cambridge. Schumpeter also regrets that Fisher cast his analysis of money into terms of the five major elements of the equation of exchange and that he buried the wide variety of the "indirect influences" behind a useless façade (pp. 234–37). But does it really "spoil" the work to summarize the infinity of forces into major variables? Is not some such summarizing necessary for the very statistical operability which Schumpeter so warmly commends? And are we not told by Fisher himself that the five variables are not necessarily graven on tables of stone but that they are a convenient rubric under which empiric investigations can proceed?

Less qualified praise is bestowed upon another outstanding empiricist, Wesley C. Mitchell. Schumpeter accounts for Mitchell's apparent hostility to theory by reference to the failure of American teachers of graduate students in the 1890's to bring out the vitality of Smith and Marshall, their concern with real social and economic processes, and their keen appreciation of objective evidence. Mitchell's resolution to let theoretic constructions emanate from "measurement" can only be applauded; but this defensible position was often linked with a kind of hostility to, or rejection of, neoclassical theory which might better also have awaited the results of measurement. Furthermore, the disavowal of theory, as Schumpeter points out, is often linked, and was in Mitchell's case linked, with the *implicit* acceptance of certain theories (pp. 247–48). None of the criticisms of Mitchell, however, is intended to impugn the originality of his great works on business cycles. Schumpeter might have added that the success of the institutionalists in America is best attested by their virtual disappearance as a school: nearly all economists accept their criticisms of traditional theorizing and are busily engaged in matching theory to institutions and other facts.

The final essay is appropriately and almost inevitably dedicated to Keynes. Schumpeter, no doubt advisedly, devotes a great deal of attention to Keynes's busy life and to his strong personal characteristics for the light they throw upon his writings. Both the *Treatise* and the *General Theory* bear the earmarks of hasty writing. Keynes was impatient of pure theory; he was no respecter of creeds or ideologies; he was a decided extrovert, and his strongest loyalties belonged to *measures*. From his earliest work to the end, "Keynes' advice was in the first instance always English advice, born of English problems even where addressed to other nations" (p. 274). Schumpeter makes excellent use of the familiar biographical device of following the chief books in chronological sequence in order to show a common core of interest, a gradual change of conviction, and the evolution of ideas finally embodied in the *General Theory;* but we cannot follow this illuminating account in any detail. Let us be content with a few of Schumpeter's main points with respect to the *Treatise* and the *General Theory*.

The *Treatise* is characterized as embracing in the theory of money the whole of the economic process, conceived in the same terms as underlay the *Economic Consequences of the Peace*. While the *Treatise* makes use of the Wicksellian divergence of money and natural rates, it already denies that thrift will mend unemployment. Liquidity preference is adumbrated in the factor of "bearishness," and the curative value of wage reductions is limited to its effect solely upon *interest* (pp. 276-78). Although the *Treatise* was "Keynes' most ambitious piece of research," its author did not believe that it effectively conveyed his message.

In essence the message to which the *General Theory* was devoted is that " 'who tries to save destroys real capital' and that via saving 'the unequal distribution of income is the ultimate cause of unemployment.' *This* is what the Keynesian revolution amounts to" (p. 290). Schumpeter does not sympathize with this tenet, but he does believe that Keynes afforded a terrific forward impulse to economic theory. What are the chief characteristics of this *General Theory?* (1) It is the outstanding (though not the first) attempt at full-fledged aggregative analysis. (2) It belongs to macrostatics rather than to macrodynamics. (3) The model belongs to the short run and particularly to depression conditions. (4) It is limited to the immediate determinants of income. (5) It achieves emphasis by giving old concepts new names (liquidity preference, marginal efficiency of capital, consumption function) and by overstatements such as the implied inevitability of price reduction following wage reduction and the complete divorcement of saving and investing. (6) Though Keynes wished to derive equilibrium with less than full employment without reference to rigidities, the infinite elasticity of the demand for money at some low rate of interest is essential to his theory, as indeed are other rigidities (pp. 281-87). Schumpeter regards this system as anything but "general" and, indeed, implies skepticism on most of its essential parts. But, "as with Marx, it is possible to admire Keynes even though one may consider his social vision to be wrong and every one of his propositions to be misleading" (p. 291).

It is one of the elements of Schumpeter's own strength to be able to admire without agreeing. Throughout this collection of essays the intellectual tolerance and benignity which characterize true liberalism make themselves powerfully evident. In part these qualities rest upon great erudition and wide and varied experience, but in part they rest upon sterling quantities of character. The book affords an intimate and pleasing view of a great economist not included among the ten which form its subject matter.

# BOOK REVIEWS

*Economic Dynamics: An Introduction.* By WILLIAM J. BAUMOL, with a contribution by RALPH TURVEY. New York: Macmillan Co., 1951. Pp. xiii+262. $5.00.

A number of recent developments in economic theory are expounded with accuracy, clarity, and simplicity in this concise and highly readable book. Professor Baumol has considerably widened the audience to whom the principal ideas of the following theorists are accessible: Samuelson on the relationship between statics and dynamics, Hicks on the equilibrium plan of the firm, Lange on price flexibility, and Harrod on growth. He provides in addition a useful primer on the mathematics of linear difference and differential equations, with numerical exercises and answers. Mr. Turvey, in a few skilful pages, introduces the student to the analysis of the firm's behavior under uncertainty and contributes a chapter on Swedish period analysis. As introduction and contrast to these topics of modern theory, Baumol presents an interesting summary of classical economics and a sketch of the theories of development of Marx and Schumpeter.

The methodological rigor and clarity with which all these topics are treated make at least as great a contribution to the reader's education in economic theory as the substance of the theories discussed. The student should gain from the book a real understanding of certain conceptual distinctions essential to modern economic theory—the distinctions between statics and dynamics, equilibrium and disequilibrium, stability and instability, and primary and secondary conditions of a maximum.

Of the two introductory chapters concerned with the older theories—in Baumol's phrase, the "magnificent dynamics"—the synthesis of classical economics is by far the more satisfying. Baumol captures the essence of classical doctrine in an ingenious but simple model. His exposition of Marx and Schumpeter is much less successful; its most serious drawback is to give the impression that these two interpretations of capitalist development are complementary rather than competitive.

Although the diverse topics of the book are very skilfully handled, they are only loosely connected with one another. No attempt is made, for example, to bridge the gap between the theory of the firm of chapter v and the theory of investment involved in the aggregative accelerator-multiplier models of chapters ix–xii. More surprisingly, no attempt is made to reconcile two different approaches to the question of the stability of a market and the dynamic paths of market price and quantity. In one chapter Baumol expounds the cobweb theorem and shows how different dynamic assumptions lead to different results; in the next chapter Turvey analyzes the short-run determination of price and quantity in the quite different language of Swedish period analysis.

Where the authors do attempt some integration, their efforts seem strained and confusing rather than helpful. Baumol makes valiant efforts to use the *ex ante–ex post* distinction, to which his colleague attaches so much importance, in the dynamic models with which he illustrates the mathematics of difference and differential equations. But the Swedish concepts are really excess baggage; the reader seeking to understand the dynamics of the multiplier, or Samuelson's multiplier-accelerator model, or Harrod's growth equations, or Metzler's inventory cycles would do better without their help. Determinate results require the specification of relationships among observable realized magnitudes at various points in time. Once these relationships, including the lags involved in adjustments, are assumed, it is a rather sterile exercise to restate them in terms of *ex ante* and expected magnitudes.

The book lacks not only the unity of a connected argument but the unity of a clear principle of selection of topics. The reader is left wondering why some topics were covered and others excluded. His judgment of the various parts of the book is bound to reflect his opinion of the importance of the contributions which they summarize. Those who share my feeling that the concept of elasticity of expectations and Lange's classification of "monetary effects" are not very useful will regret the space devoted to these topics.

In its major area, aggregative dynamics, the book is more instructive in mathematical mechanics than in economic analysis. Many students of economics will find the last four chapters the best available introduction to the mathematics of linear dynamic systems. But they will find little consideration of the economic logic behind the equations of the dynamic model discussed. From this standpoint, it is unfortunate that this book, which is based on lectures given in 1947-49, could not have taken account of the modifications of the acceleration principle which Hicks, Goodwin, and Leontief have introduced and of the nonlinearities to which these modifications lead.

JAMES TOBIN

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*The Theory of Investment of the Firm*. By FRIEDRICH and VERA LUTZ. Princeton: Princeton University Press, 1951. Pp. xx+253. $4.00.

This is an important book. In it the Lutzes cover, in systematic and elegant fashion, traditional problems of investment theory as applied to the business firm. Much new light is thrown on old and perplexing problems. Beginning with the relation of the time element to ordinary production theory, the authors proceed to an analysis of recent discussions on the objectives of maximization. In this manner they are able to isolate, if not resolve, much of the confusion that has plagued this literature. The Lutzes list four criteria of maximization: (1) the difference between the present value of future revenue and cost stream, $V - C$; (2) the ratio of these present values, $V/C$; (3) the internal rate of return on total capital invested; and (4) the rate of return on the entrepreneur's own capital. As they gently remark: "It is doubtful whether all authors have realized that different results may be obtained if one criterion is adopted rather than another. The explanation is probably that in competitive general equilibrium, when an entrepreneur just earns the going interest rate on his investment, all four criteria amount to the same thing" (p. 17).

Although mindful of circumstances where other criteria may become more rational, the Lutzes plump for $V - C$, which is in most cases equivalent to maximization of the rate of return on the entrepreneur's own capital. Following Fisch's classification, the authors proceed to analyze various types of production and investment problems: (1) the point-input-point-output type (with the investment period fixed and variable)—a crop, tree, forest, etc.; (2) the continuous input-point-output type—work-in-process inventory, etc.; and (3) the point-input-continuous-output type—durable goods. Their treatment encompasses relationships to the trade cycle; to time, funds, and investment horizons; to scale considerations; and to risk, uncertainty, and liquidity.

There is scarcely a section of the book which cannot be read with profit. But the book is not easy reading. Given the elegance of the formulations and the systematic method of attack, the book is surprisingly difficult. One suspects that the demands of book presentation did not weigh heavily in translating into print the material from Lutz's course at Princeton. Distinctions between behavior, structure, and definitions do not seem to bother the authors. It is not that they are unaware of these distinctions (there is ample evidence that they are); it is merely that they do not seem to feel it worth their while to make these distinctions clear to the reader. Even in the introductory equations dealing with the criteria of profit maximization (chap. ii) one needs to notice that $\rho$ (the internal rate of return) is a function of $C$, while $r$ (the interest rate) is not, or else the analysis reduces to a tautology or to nonsense. Careless statements and omissions in the literary discussion also make the reading more difficult than it need be. But since clarity and completeness usually follow in the mathematical presentation, one should not carp at the Lutzes' execution of what is, at best, a difficult task. It is better to have the book available now. Filling and polishing can await some future revision.

The book requires elaborate classroom supplementation, even for fairly advanced students. It is what might be termed a "sound" book in theory. It possesses neither the stimulation of Boulding's *A Reconstruction of Economics* nor the applied and empirical orientation of Terborgh's *Dynamic Equipment Policy*. Unlike Terborgh's work, also, this book is not concerned with the behavior of the *individual* firm. Its concern is rather with the traditional construct of the firm as oriented toward industry- or economy-wide analyses. It is of interest in this connection that what the Lutzes call "cost accounting" (see chap. iv and *passim*) would more appropriately be called "budgetary estimation." The "control" features and the "principle of exceptions," on which modern cost accounting systems revolve, find no mention in this volume. Questions of adaptation for practical application (which weigh heavily with Terborgh) and questions of information, processing cost, and relevance (such as those treated in the "separation theorems" of Leontief and Modigliani) are also lightly treated or ignored. The peculiarities of individual firm equipment replacement policies and practices as described by Terborgh are touched upon but lightly. On the principle of statistical averages, it may be the case, of course, that *all* firms behave in a manner analogous to that described by the Lutzes, despite the fact that no firm in particular conforms to this pattern.

Whatever the merits of this argument may be, the book needs no apology to an audience of economists. It will stand for a long time as an authoritative tract in what it attempts to do. Systematic and complete, the book is also circumscribed. It does not, like Hurwicz, for example (*Econometrica*, 1946), attempt to extend the analysis of entrepreneurial behavior (through the concept of a utility function) to consumption (and, hence, savings) as well as production and investment.

W. W. COOPER

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*The Industrial Economy: Its Technological Basis and Institutional Destiny.* By C. E. AYRES. Boston: Houghton Mifflin Co., 1952. Pp. x+433. $4.00.

"Institutionalism" as a separate system of thought has almost disappeared from serious economic discussion. It did not disappear because of the wilful design of an organized group interested in the preservation either of the existing social order or in the mathematical character of the established discipline. Institutionalism has been discarded because it offers little that is constructive. Even the value of its occasionally penetrating criticism is reduced when it is recognized that such criticism stems, in part, from a basic misunderstanding of "orthodox" thinking, both substantive and methodological. This misunderstanding is reflected in this work, the latest item in Professor Ayres's continuing controversy with orthodoxy (i.e., almost everyone else).

The industrial economy of Ayres's construction is inherently unstable. With industrial development, the rich get rich faster than the poor. This leads to an increasing disparity between savings and investment. The major policy step required to correct this is progressive income taxation (positive and negative). Consumption will then be increased sufficiently to remove the disparity, and capitalism will no longer have to resort to war to rid itself of its surplus production. Little attention is given to the efficacy of redistribution policy and achieving the stated goal of higher consumption; recent empirical studies are not cited. The significance of the difference between the average and marginal propensities to consume is overlooked. The symmetrical argument for regressive taxation and expenditure in periods of inflation is not included. The date of publication is 1952, but there is practically no recognition of the realities of the postwar world.

In the discussion of price theory an appropriate question is asked: "What do all the equations mean?" (p. 337). The ability of the author to answer his own question may be estimated from the following: "Goods do not occur at the expense of each other" (p. 403). "Before anything can be bought and sold, it must first exist. What is available to be bought and sold cannot be greater than what exists; and what exists depends on the state of the arts and crafts" (p. 88).

"The idea of capital as a creative agent . . . is a sort of myth" (p. 118). Economic progress results from growth in the industrial apparatus, which is not brought about by saving. Saving brings into existence only money. This argument is extended even to the Crusoe case. Physical capital is not formed as a result of saving for how could Crusoe "pick a whole lot more" berries than enough to keep him alive, and besides "berries spoil quickly." Crusoe builds a boat because of the "know-how" that he has and with the tools salvaged from the shipwreck (presumably fasting during the production period).

"We are able to conceive the general welfare because we can see that good milk makes well babies" (p. 305). Would the problem were so simple. Good milk makes well babies, but it also keeps the aged alive. Does the general welfare dictate allocating all the milk to babies or some to the aged (a problem actually faced in postwar France)? These are the hard problems, and choice in terms of basic values cannot be avoided. I fail to see how these choices can be made except by individuals on the basis of some ethical schema. And democracy implies that all individuals shall participate in the decision-making.

The existence of mutually conflicting alternative ends is characteristic of all the really important social problems, and the failure to recognize this reflects not only wrong but dangerous thinking. This failure is the most pervasive element in the book. The myth of capital vanishes once it is seen that "eating up" and "investing" are always potential alternatives; the scientific determination of the general welfare is no longer possible when it is understood that many socially desirable goals conflict with each other; the price system is meaningless until the elemental notion of opportunity cost is grasped. The author admits that scarcity is present in the sense that all goods are not free but says that this is scarcity only in the individualistic sense; for society resources are not scarce but abundant. The whole is less than the sum of its parts.

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*The Ethics of Redistribution*. By BERTRAND DE JOUVENEL. Cambridge: Cambridge University Press, 1951. Pp. vii+91. $1.75.

This little book reproduces two thoughtful lectures given at Corpus Christi College, Cambridge, in 1949. In what the author (a nonprofessional economist) characterizes as "rather a circumgyration around the concept of redistribution than an argument," Baron de Jouvenel stresses the social and ethical issues at stake in measures to equalize incomes. "Redistribution" is defined as "taking from the higher incomes to add to the lower" by means of state expenditure "which relieves the individual of an expenditure that he could and presumably would have undertaken out of his own purse."

The author concludes: (1) Redistributionism (and contemporary socialism, which embodies it) is not a legitimate offspring of the ethics of charity, equal capital endowment, and brotherly love that characterized the church, agrarian reformism, and classical socialism. (2) Redistribution is fatally defective ethically, because it glorifies the "pursuit of ever-increasing consumption," thus lacking the vital "contempt for worldly goods" which made these ideals consistent. (3) The "floor" below which it is nowadays felt incomes should not be permitted to remain, and the "ceiling" below which it is felt incomes should not be reduced, are far too high to be "financially harmonious"—that is, there is not enough income above the ceiling to raise all incomes to the floor. Consequently, (4) the pursuit of redistributive policies involves far more *horizontal shifting* of the objects of consumption *within* income layers than of income between them. (5) Since this process involves in the main a shift from private to state expenditures, redistribution is essentially "a redistribution of power from the individual to the State." Finally, (6) these policies deny the family, the truly basic social unit, the opportunity and responsibility to provide for its own cultural advancement. These last two points alarm De Jouvenel especially.

The author's argument (for there *is* much good argument, despite his protestation) will convince nearly everybody that (*a*) absolute equality of incomes is socially and ethically undesirable, as well as economically inexpedient; (*b*) the theoretical case for it built on the "equal satisfaction" principle is full of pitfalls; (*c*) state assumption of all expenditure on personal development and cultural expression is socially obnoxious; and (*d*) the prime economic problem of *most* countries is still too small a pie rather than a bad division of it.

But the argument reaches too far. Even "welfare state" partisans do not generally call for (*a*) or (*c*). Nor do they deny (*d*). Moreover, welfare expenditures are *not mainly* designed to redistribute incomes: a substantial portion are to relieve distress; a still larger part are directed to make *opportunity*, rather than incomes per se, more equal. These ethical principles De Jovenel accepts. Furthermore, the great bulk of state expenditure which when financed by progressive taxation reduces income disparities is not aimed at "redistribution" at all but at providing military and civil protection. To take as large a portion of low as of high incomes to finance this huge expense would run counter to even stronger ethical principles than De Jouvenel defends.

Finally, is not the author wrong in thinking that modern economists believe in greater income equality "on the ground that absorbing the 'stuff' [consumers' goods] is the staff of life"? Is not more typical the mere recognition that bread is the *staff* of life, though men live not by it alone? Social measures to insure it to all have strong prima facie merit, ethically and in terms of those "contributions to civilization" for which Baron de Jouvenel rightly pleads. As Lord Keynes said, "Economists are the guardians, not of civilization, but of the *possibility* of civilization."

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*Ausgewählte Texte*. By JOHANN HEINRICH VON THÜNEN. Ausgewählt und kommentiert von PROFESSOR DR. WALTER BRAEUER. ("Die grossen Sozialökonomen," Band VIII.) Meisenheim/Glan: Westkulturverlag Anton Hain, 1951. Pp. lxii+309. DM 29.

Contained in this volume are an abridgment of *Der isolierte Staat*, six articles or letters by Thünen, a fifty-three-page study of the author, and two bibliographies, one of writings by, and the other of writings about, Thünen. It is the first volume issued in a projected series called "Die grossen Sozialökonomen."

The major part of the book is a reprint of roughly one-half the complete text of *Der isolierte Staat*. Since there are readers who shun abridgments, this part of the book will not be received cordially everywhere. The editors must have hoped, however, that the shortening would win more readers than it would lose. This hope had a reasonable base, for the complaint has been heard frequently that the full text is prolix.

The most striking part of the book is the essay on the life and work of Thünen. At every point it impresses the reader with the extent to which Thünen is studied in Germany. Professor Walter Braeuer, the author of the essay, is only one of many economists in Germany who have specialized on Thünen, and he draws on the work of such other specialists as Hermann and Robert Schumacher, Asmus Pertersen, Richard Ehrenberg, Hermann von Wenckstern, Richard Krzymowski, and F. Lifschitz. Together they form the core of a Thünen cult, which has flourished under the many different circumstances in which Germany has found herself in the last seventy-five years. Thünen is, and has been, deservedly in the hands of friends, and certainly an economist with twenty doctoral dissertations written about him and a street in each of five German cities named after him has had influential friends.

The two bibliographies at the end of the book are helpful and particularly welcome, since bibliographies in economics are so few relative to the number found in other fields of study. The one that lists the printed and manuscript writings of Thünen must be nearly as complete as bibliographies are generally permitted to become, both because it is the end product of a series of bibliographies which began with the one Hermann Schumacher drew up in 1863 and because it was prepared by Hermann von Wenckstern and his wife, who are both close students of Thünen. It contains 129 items, of which 6, thought to be of importance and up to now unpublished or published in relatively inaccessible places, are reprinted as part of the present volume. They merit whatever reception is to be given the minor writings of a famous author.

The second bibliography, of works about Thünen, is taken from the cards of the bibliography at the Thünen-Archiv at the University of Rostock. While admittedly and obviously incomplete, it reflects in still another way the intensity of the study of Thünen in Germany, for it contains over six hundred items, of which not many more than twenty are in languages other than German.

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AMERICAN INSTITUTE OF ACCOUNTANTS. *Report of Study Group on Business Income: Changing Concepts of Business Income*. New York: Macmillan Co., 1952. Pp. ix+160. $2.00.

In 1947 the American Institute of Accountants matched a grant from the Rockefeller Foundation to undertake a three-year study of the concepts and terminology of business income, with particular reference to the impact of changing price levels on the measurement of such income. The AIA established a study group of some sixty accountants, economists, lawyers, and businessmen, an executive committee of six, with Percival F. Brundage as chairman, and a drafting committee directed by George O. May. The present volume is the final report of the study group and embraces its conclusions, supporting material prepared by the drafting committee, and a number of dissents and comments by members of the group. Although the members who sign the final report do not necessarily subscribe to the supporting materials, the volume is apparently intended to be regarded as an integrated treatment of the subject.

The starting point is that accounting rests on certain postulates and assumptions whose validity must be judged by their usefulness and not by their quality as "demonstrable truths." In current accounting procedure one of these postulates is that "fluctuations in value of the monetary unit, which is the accounting symbol, may properly be ignored" (p. 20). The usefulness of this postulate must be re-examined since it no longer squares with the realities of economic life. Business income is, in general, realized gain. Gain is the excess of revenues over "costs." The problem is the measurement of "costs," and the study group finds that, "ideally, costs should be measured by standards as nearly identical as possible with those by which revenues are measured" (p. 104). In practice the choice of units of measurement lies between "*monetary units of changing purchasing power* and *units of equal purchasing power* (p. 104; italics in original).

The study group finds support for the adjustment of costs to current price levels in the increasingly widespread adoption of LIFO methods for inventory and in accelerated amortization for defense facilities. This experience *might* be extended by the use of replacement costs for asset valuation or by the application of an adjustment for the change in the purchasing power of the monetary unit. The study group concludes guardedly: "Corporations whose ownership is widely distributed should be encouraged to furnish information that will facilitate the determination of income measured in terms of approximately equal purchasing power, and to provide such information wherever it is practicable to do so as part of the material upon which the independent accountant expresses his opinion" (p. 105).

It would be difficult to take exception to this conclusion worded in these terms. Almost all parties interested in the determination of business income would be happy to have available additional information on the character of business income, computed on a number of different bases. But this is the trouble with a report of a study group: to produce a summary that will enlist the maximum of adherence, it is apparently necessary to equivocate. In this case the equivocation is such that, judging from the individual comments, some members of the group felt that the document supported a replacement basis of asset valuation, and at least one member felt that it supported the original cost basis of valuation. The overtones of the supporting material and the conclusions are, however, unmistakable. The document as a whole lends support to the replacement basis or some equivalent adjustment.

In fact, this conclusion is inevitable if costs and revenues are to be measured on the same basis. Since revenues can be measured only in terms of current prices, costs must also be so measured. It follows that inventories must be valued by LIFO methods and that depreciable assets must be valued in terms of current replacement costs.

When the matter is put in these terms, the controversy is likely to be sharpened. The measurement of inventory values on LIFO and of asset exhaustion on the basis of replacement would provide the business community with a considerable hedge against inflation. It would also reduce business income-tax liabilities during inflationary periods, thereby reducing the stabilizing influence of the tax in such periods, and store up increased tax liabilities for periods when prices and business activity declined.

There can be no doubt that at least some portions of the business community need protection from the impact of a rising price level. But so do other economic groups whose relative position is impaired far more than that of the business sector. As one of the dissenters (Charles W. Smith) put it: "We would accomplish much more by devoting our efforts to the prevention of inflation and deflation rather than by attempting . . . to apply a palliative designed to soothe only one or two of the symptoms" (pp. 127–28).

The conclusions of the study group may not be provocative, but the issues are. The supporting materials, comments, and dissents get most of these issues out in the open.

JESSE BURKHEAD

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*Typologie der Theorien des Industriestandortes.* By HANS ULRICH MEYER-LINDEMANN. (Akademie für Raumforschung und Landesplanung Abhandlungen, Band 21.) Bremen-Horn: Walter Dorn Verlag, 1951. Pp. 240. DM. 8.00.

This is a well-organized and amply documented Baedeker to the literature of industrial location theory. The author has summarized and classified the theories of more than a hundred and twenty previous writers and has appraised their soundness and their utility as policy bases.

His survey shows that interest and emphasis in locational theory have progressively shifted from descriptive analysis of the problems of the cost-minimizing or profit-maximizing individual firm to normative analysis of the problems of the whole economy in terms of broad long-range criteria of social welfare.

In the policy discussion the line between the author's own views and his presentation of the views of other writers is often impossible to discern. The closest approach to a real statement of the basic criteria of "sound" structure and policy begins on page 167, in terms of "full employment" and "stability." "Full employment" here includes the opportunity for each individual to use and develop all his capabilities and lead a well-rounded life. "Stability" is broadly defined to include not merely protection against cyclical unemployment but also a settled way of life with minimum migration and commuting.

These two aims, "full employment" and "stability," appear to be accepted axiomatically as best for both the individual and the economy. Technological progress and productive efficiency appear to be completely ignored as objectives or criteria.

Since the more "capitalistic" sectors of the economy are regarded as incurably more vulnerable to crises than sectors like general farming and local trade, the objective of "stability" is invoked to support a policy of decentralizing industry and fostering the rural way of life. A favorite phrase to describe the ideal is "enge Wirtschaftskreisläufe."

Antipathy to the modern city and nostalgia for the settled rural way of life reach a fanatical pitch in several of the writings cited, with seeming approval, by the author.

The two worst bogeys are "Blutchaos" and "Verstädterung." The former refers to the genetic results of excessive migration. When people move about, family and neighborhood ties are broken, and the mingling of blood stocks from diverse places hinders natural eugenic progress that might result if people would stay put and intermarry locally. The author uses as his authority for this assertion a German investigation (1942) which purported to show that metropolises produce less than their proportionate quota of geniuses. Even a reviewer largely innocent of knowledge in this field may suggest that inbreeding may produce not merely more geniuses but more defectives and that historical evidence might justify ascribing a good share of our economic and social advances to the very mobility, migration, and intermingling that the author so deplores. Perhaps part of the conflict here stems from the fact that he gives stability overriding priority and appears to take progress for granted.

"Verstädterung" refers to the objectionable effects on the individual resulting from his moving to the city or even commuting to it. Aside from the time and discomfort involved in getting to work in the city, the individual becomes prey to radical ideas and loses the close neighborhood ties of the small town. No mention is made of any reasons why some people prefer city life when they have a choice. Some feel, for example, that the "close neighborhood ties" of small-town society are not an unmixed blessing.

The foregoing comment on some of the basic concepts underlying the policy-oriented sections of the book illustrates the wooly and subjective character of most of the existing literature on locational planning and policy. It would be unfair to blame the author for the collective shortcomings of the writers he discusses. One could wish, though, that he had been less tolerant and had carried through the latter parts of the book the objective critical acumen that characterizes the first part.

EDGAR M. HOOVER

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*The Quantity Theory of Money.* By HUGO HEGELAND. Goteborg: Elanders Boktryckeri Aktiebolag, 1951. Pp. x + 262.

In this full-length monograph Hugo Hegeland considers in chronological order what writers from Confucius to Keynes have had to say about the factors determining the value of money. He appears to argue that the quantity theory of money has usually been misinterpreted and that, if only "active" money (in contrast to what he terms "passive" money) were considered, the quantity theory would be a useful generalization. He also seems to argue that Keynes has ignored the role of "active" money in the economy. Finally, in his last chapter he contends that Keynesian and classical monetary theory can be reconciled by utilizing a system of equations designed to show how changes in the distribution of money between "active" and "passive" balances will determine the national income.

The major part of this monograph is concerned with tracing the historical development of the quantity theory of money. This could have been an extremely useful piece of work, particularly if Hegeland had been concerned with the gradual transformation of the quantity theory to an equation-of-exchange explanation of the factors determining the price level. But Hegeland's work suffers on a number of counts. It is unbalanced in emphasis. Curiously, he omits any discussion of the contributions of John Law, Henry Thornton, and W. Stanley Jevons. His discussion of John Locke and David Hume is much more extensive than his discussion of D. H. Robertson and R. G. Hawtrey. Moreover, it is extremely difficult to understand what Hegeland has to say about the writers he does include. This is partly because of his inexact use of the English language and careless proofreading. Many of his sentences are unintelligible, and his argument is difficult to follow. But, most important, in place of carefully explaining each writer's position and appraising his place in the development of the history of monetary theory, Hegeland seems to be primarily concerned with producing a series of caricatures which fit into neat categories. As a study in the history of economic thought, Hegeland's work is disappointing.

Hegeland's attempted synthesis between the quantity theory of money and Keynesian monetary theory is equally disappointing. He asserts, without examining the evidence, that the velocity of active money is constant in the short run. (Active money is money that is spent at least once during the period.) He does not consider the factors which determine the distribution of money between "active" and "passive" balances. In criticizing Keynes's alleged failure to consider money as a medium of exchange, he ignores Keynes's discussion of $M_1$ and $L_1$. Hegeland treats shifts from "active" to "passive" balances as if they were autonomous and not related, as the neoclassical economists argued, to the economizing decision of persons.

There is a need for careful studies of the growth and development of monetary theory over the last two centuries. There is also a need for testing hypotheses derived from competing bodies of monetary theory. Hegeland's monograph does not satisfy either of these two requirements.

E. T. Weiler

*University of Illinois*

*Commercial Bank Activities in Urban Mortgage Financing.* By Carl F. Behrens. New York: National Bureau of Economic Research, 1952. Pp. 152. $2.50.

This monograph is an analysis of a sample of urban real estate mortgage loans made in the period 1920–49 by one type of financial institution, the commercial banks, which held in the aggregate a total of $10.5 billion urban mortgage loans in the United States, or 18.1 per cent of the total amount held by all institutional and private lenders. The chief emphasis is upon loans on residential properties, which constituted 81 per cent of the dollar amount of the urban mortgage loans of the commercial banks. The period of time selected for the study was one characterized by rapidly rising real estate prices in most American cities from 1920 to 1925, a period of dulness from 1926 to 1929, a sharp decline accompanied by heavy foreclosures from 1930 to 1933, and a rise, gradual at first but then gaining momentum, from 1934 to 1949. In view of these trends in real estate prices, it would be expected that foreclosure rates would be highest for the period 1925–29 on mortgage loans made at the peak of prices just on the eve of a severe drop in values. The foreclosure rate of 11.1 per cent for urban mortgages made by the commercial banks from 1925 to 1929 compared with 2.6 per cent for the 1920–24 mortgages, 1.2 per cent for the 1935–39, loans and practically none for the 1940–47 period confirms what one would anticipate on the basis of a priori reasoning (Table 19, p. 62).

The liquidation experience indicated that the highest losses incurred were on loans liquidated and sold in the period 1940–47, which caused the author to state: "The principal conclusion to be drawn is that for the period covered properties held for the longest time, regardless of when the loans on the foreclosed properties were made, showed the greatest eventual loss" (p. 66). This conclusion is surprising in view of the rapid increase in real estate prices in most cities from 1944 to 1946, which should have enabled the banks to recover their investment. The explanations suggested by the author account for some of the causes for losses on properties held the longest. First, the properties were costly to operate and, second, the best properties were sold first, leaving the less desirable properties to the last (p. 66). One very important factor, however, was not considered. In view of the rapid change in the real estate market in the period

1940–47, the experience of loans foreclosed and sold from 1944 to 1947 should have been put in a separate category from those sold from 1940 to 1943. Possibly an entirely different conclusion would have been reached if there had been an adequate sample of loans in this last period.

Mr. Behrens' study presents the best statistical evidence available on important characteristics of the urban mortgage held by commercial banks, such as the decline in contract interest rates, the rise in contract mortgage lengths, the increase in loan-to-value ratios as a result of FHA guaranteed mortgages, the increasing trend to amortized loans, the extent to which mortgages have been modified, and the cost of servicing urban mortgages. More could hardly be expected in a volume of this length. In order to understand the causes of the urban mortgage loan experience of the commercial banks, however, some background of the course of urban real estate prices should have been presented either here or in a separate volume. This monograph is one of a series of urban real estate finance studies completed under the financial research program of the National Bureau of Economic Research, and the author has made a careful statistical investigation of the subject assigned to him. This monograph will be of great value to students analyzing urban real estate mortgages or the investments of commercial banks.

HOMER HOYT

*Chicago, Illinois*

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*Central Banking in Undeveloped Money Markets.* By S. N. SEN. Calcutta: Bookland, Ltd., 1952. Pp. vi+246. 18*s*.

The undeveloped money markets of Dr. Sen must not be confused with the so-called underdeveloped countries of current economic literature. Only London, and possibly New York, are specifically excluded by Sen's criteria. While other European countries are not mentioned, his discussion of undeveloped money markets extends indiscriminately to such strange bedfellows as India and Argentina, Paraguay and Australia, the Philippines and Canada, etc. Sen explains the inefficacy, in such markets, of the more traditional instruments of central banking—primarily rediscounting and open market operations—and evaluates the merits and demerits of other techniques such as direct lending by the central bank, variable reserve ratios, and selective credit controls. This discussion of techniques is often interesting, and the book is well written and well organized. It marks an obvious progress over some older attempts to idealize and sterilize into a universal and perennial code of central banking the rules and traditions empirically developed in nineteenth-century England to fit the highly special needs and opportunities of the world's greatest trade and financial center. This necessary reorientation of central banking theory, however, is not new, and Sen's book does not compare in interest or realism with the work of some of his predecessors, such as A. F. W. Plumptree or William Adams Brown. The distinction he introduces is far too crude in view of the enormous differences between the countries cloaked together under his "undeveloped money markets" label. Such differences can be ignored only at the cost of keeping the analysis at a highly abstract level, examining each technique per se in a complete vacuum, with little or no reference to the system as a whole or to the broad economic objectives which such techniques are to serve.

There is, indeed, no discussion of policy objectives beyond the platitudinous statements that the business of a central bank "is not . . . to earn profits for its shareholders, but to safeguard the stability of the monetary and credit system of the country" (p. 8) and that "the fundamental task of a central bank is to control the activities of commercial banks" (p. 13). The only other explicit reference to policy questions is that "in the undeveloped economies, and indeed in the industrially developed countries as well, the functions of central banks have not ended with the achievement of control over the commercial banks. They have, in recent times, been obliged to assume responsibility for encouraging, and even undertaking the financing of the development of the resources of the country" (p. 204). The latter statement introduces, in the last chapter of the book, a very brief discussion of central bank aid to rural credit and to the supply of long-term finance to farmers and industry, mostly in India, Australia, and Canada.

Exchange controls are specifically excluded from the subject matter of the book, but the broader problem of harmonization between domestic policy objectives and external balance is also excluded from consideration. Only by inference can we guess that Sen generally favors neutralization policies rather than the "rules of game" of neoclassical writers. I cannot remember a single mention of exchange rates in the whole book.

Empirical data and references to legislation are too scattered and sketchy to be of much use. Most of them are understandably derived from British Commonwealth countries, and particularly Australia, which has, more than others, experimented with new techniques in this field. Nowhere, however, do we get any over-all picture of the system or its results, the discussion being in each case narrowly confined to the particular technique analyzed. Information on Latin America is especially unsatisfactory and incomplete: references to legal provisions are haphazard, and no quantitative data are offered, with the single exception of a few pages on the neutralization policies of the Argentine Central Bank in 1935–39. The two most revolutionary banking reforms of recent years—in Argentina in 1946 and Ecuador in 1947—are not even mentioned.

This is, however, a Ph.D. dissertation. I very much hope that Sen will soon use his excellent training and evident ability to give us a different kind of book, integrating techniques with the policies they are to serve and relating both to the concrete problems and environment of some well-defined country or area rather than to the indiscriminate jumble of all money markets different from nineteenth-century London.

ROBERT TRIFFIN

*Yale University*

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*Bilanzbildsequenzen.* By ERWIN FEIN. Bern, Switzerland: A Francke AG Verlag, 1951. Pp. 168.

This work attempts to utilize the double-entry bookkeeping methods of the individual firm for an analysis of the economy as a whole. Basing his conclusions on carefully developed hypothetical cases, Dr. Fein works out profit-and-loss statements for the various segments of an economy under certain assumed and, admittedly, oversimplified and unrealistic conditions. Though starting with a static economy model, he is mainly interested in finding a dynamic equilibrium and in determining the forces that interfere with it.

Fein attempts to show that the same national income, if differently distributed, would lead to a different, derived demand schedule for capital goods, not only, as Keynes reasoned, because of an increase in the marginal propensity to save for an economy with a more unequal distribution of income but also for the (rather unrealistic) assumption of an unchanged marginal propensity to save, irrespective of the magnitude of individual incomes. This conclusion is based on the probably correct assumption that a more equal distribution of income would result in a relatively greater consumption of commodities and a relatively smaller consumption of services, with the resulting difference in the derived demand for capital goods. In this way Fein reaches the conclusion that an increase in productivity could only result in a comparative increase in actual output if the distribution of additional income were such as to call forth additional investment (i.e., if a relatively large part of the additional national product went to labor). Should this be the case, however, entrepreneurs in a free-enterprise system would have little incentive to expand production.

In conclusion, Fein proposes the replacement of countercyclical, compensatory taxation and public work measures by a system of forced business reserves and blocked consumers' bank accounts during inflationary periods to be released during periods of threatening depressions. Thus, the countercyclical, compensatory measures that he suggests offer much greater opportunities for individual investment and consumption decisions (during periods of recession and depression) than those envisaged by Keynes and Hansen.

Without trying to evaluate the merits of the conclusions reached, I cannot help but feel that the same could have been accomplished without the wide use of the double-entry bookkeeping methods employed in this brief volume. It seems to me that the vast mass of data in an entire economy makes the use of accounting methods extremely difficult if not impossible. On the other hand, the use of statistics for the compilation of data and the subsequent presentation of total figures for large segments by means of balance sheets and profit-and-loss statements seem of questionable merit. I fail to see how it would contribute directly to an explanation of causal relationships—it might indirectly through clearer presentation of data.

Fein admits all these difficulties and points out that he only attempts to lead the way toward a new method which others may develop sufficiently for it to prove useful in clarifying economic problems.

HARRY SHAFFER

*University of Alabama*

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*Multiple Exchange Rates and Economic Development.* By EUGENE RICHARD SCHLESINGER. Princeton: Princeton University Press, 1952. Pp. 76. $1.00.

The second of the "Princeton Studies in International Finance" for investigations which are more than articles but short of full-fledged books, Dr. Schlesinger's précis of his doctoral thesis deals with a timely topic. The rash of experiments in multiple exchange rates in Latin America, first encouraged from Washington by the Federal Reserve System and now mildly discouraged by the International Monetary Fund, provides a wealth of material for analysis.

It is something of a disappointment, however, to report that Schlesinger's treatment of the subject is less than definitive. In a sense, the author falls, beside vast numbers of his fellow-economists, between the stools of the inductive and the deductive approaches. What starts out as an empirical investigation of Latin-American multiple-exchange-rate experience ends up as an almost exclusively theoretical treatment of the possible relations running from multiple exchange rates to imports, national income, the level of exports and foreign investment, and the geographical distribution of trade. Case material on Peru and Argentina, provided under this last topic, is limited in scope and fails to answer many questions.

As a theoretical work, the book has strengths which make it useful for supplementary reading by graduate students. Undergraduates will have difficulty with the elliptical compressed style. On the theoretical level, however, it is to be regretted that the author did not try to generalize more widely and along a less mechanical outline. To what extent, for example, is the major advantage of multiple exchange rates to be found in their effects on the terms of trade through enabling countries to act as a discriminating monopolist/monopsonist? To what extent, further, is the effectiveness of multiple exchange rates dependent upon lack of retaliation?

A great deal of economic discussion is at cross-purposes because the disputants are comparing a given proposal against different standards: the pessimist against the ideal; the optimist against the realities of the world. Any disappointment over this book's failure to meet the ideal should not obscure the fact that it makes a real contribution to the sparse literature on multiple exchange rates.

C. P. KINDLEBERGER

*Massachusetts Institute of Technology*

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*International Cartels, Economic and Political Aspects.* By KENNETH L. MAYALL. Rutland, Vt., and Tokyo, Japan: Charles E. Tuttle Co., n.d. Pp. x+173. $2.75.

This little book (138 pages plus a bibliography and 20 pages of appendixes) was written in Japan during the author's six years' employment on the staff of SCAP. Of the six persons whose advice Mr. Mayall acknowledges by name, five are Japanese, adherent before the war to the dominant economic and political group. A preface by another prominent Japanese refers to Mayall's services as "guest lecturer to economic discussion groups comprised of Japanese business leaders, both purged and unpurged, and scholars of international economics and finance" and describes Mayall as a realist who "intuitively knew that the pattern of Japan's phenomenal economic development during the preceding three decades would be duplicated, possibly within the decade on the immediate horizon, and probably through resuscitation of prewar business and financial techniques." The author's prefatory note apologizes for lack of page references in his footnotes on the ground that the source material available to him in Japan and the fragmentary notes of his earlier researches were insufficient to permit accurate citation.

From this setting, Mayall has chosen to write on the historical development of industrial combination and cartelization throughout the world and the differences in national policies applicable thereto. His book reflects familiarity with earlier books and the more readily available public documents; but, as was to be expected, it is deficient where interpretative background is needed, where the important documents, though public, are hard to get at, and where, as in European attitudes and legislation, there has been a significant change since 1945.

Mayall takes it for granted that the alternatives before the world are private cartelization or publicly established restrictions on trade, and of these two he prefers the former. He advocates international registration of cartels with an agency authorized to regulate and review such private agreements. Nevertheless, he expresses doubt that supervised cartels will function differently from unsupervised ones and, indeed, asserts that "cartels admitting of government participation pose a greater threat to a competitive economic system than do unsupervised agreements" (pp. 121–22).

It is regrettable that Mayall did not focus his attention primarily on Japan. His contacts with Japanese business leaders, his acquaintance with the development of the policy of the occupation toward the *zaibatsu*, and his sympathy for controlled cartelization gave him an unusual opportunity for an original analysis. Unfortunately, the eighteen pages which he devotes to Japan are spent upon generalities, some of which are so presented as to be misleading. Thus the *Report of the Mission on Japanese Combines* is described as "idealistic" and is said to have been subsequently "disavowed"; and the Yasuda plan for deconcentration is said to have been "later" submitted for approval by the occupation authorities. The Yasuda plan was actually approved by the occupation authorities before the Mission on Japanese Combines left the United States; and the task of the mission was to report whether or not the plan needed supplementing. The greater part of the mission's report was adopted and executed. Far from being idealistic, the report centered upon measures that were needed in order (*a*) to expedite the resumption of private management of *zaibatsu* companies and thereby restore the productive efficiency of those companies and (*b*) to prevent the subsequent nullification of the program either through business intrigue or through reversal of the policy by the Japanese government. In noting that the Mission's recommendations were not fully executed, Mayall also records his belief that the Mission's forebodings are now becoming realities.

CORWIN D. EDWARDS

*Washington, D.C.*

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*Formulation and Economic Appraisal of Development Projects*, Vols. I and II. New York: United Nations Publications, 1951. Pp. xiv+780. $3.00 and $2.50.

Loan funds and loanable experts do not make a technical assistance program. This was learned early in the "Point IV" game. Projects submitted to the International Bank, for example, have often been so poorly conceived and formulated that they could not seriously be considered for external financing. To help narrow the gap between the developmental aspirations and the planning "know-how" of the economically underdeveloped areas, the United Nations, the Food and Agriculture Organization, and the International Bank have jointly sponsored three regional study "centers" devoted to the problems connected with the formulation and evaluation of programs and projects. The first of these was held at the end of 1950 in Lahore, Pakistan, for officials from the Asian countries. Similar three-month courses were given at Ankara, Turkey, and at Santiago, Chile, at the end of 1951. The two volumes under review contain the lectures given at Lahore by the experts provided by the sponsoring organizations, Volume I covering the "major courses" and Volume II covering the discussions of "special problems of economic development."

The lectures are presented just as they were given, so that they are in the form of colloquial, discursive discussions, with the freshness—and looseness—characteristic of largely extemporaneous teachings. The lectures cover a wide variety of subjects, from general principles of economic development and broad questions of organization, administration, planning and finance, to specific techniques of project formulation and evaluation and methods of economic forecasting.

At their best, the lectures provide searching insights into the problems of economic progress and developmental planning, in some instances signifying the breaking of new ground. At their worst, they contain a rehash of readily available materials whose applicability to underdeveloped areas is, under any circumstances, extremely doubtful.

Hans W. Singer, in the opening lecture, discusses a number of principles which should govern the design of national programs to increase productivity and raise levels of living. His lecture is a storehouse of miscellaneous suggestive materials and valuable illustrations, but it lacks the integrating force of a theoretical framework for treating the central questions of economic development, a lack of which Singer himself is well aware. Mordecai Ezekiel, currently with FAO, in a remarkably concise statement, describes the established methods of economic forecasting.

Rather surprising is the fact that, of all the lectures, only Egbert de Vries, of the International Bank, refers to national income accounting in discussing techniques of developmental planning. It seems strange that so little use should have been made of a tool of economic analysis and planning whose value has been so widely demonstrated.

Much of the discussion in both volumes centers on the problems of project evaluation, on methods for appraising the costs and benefits of development programs and projects. All the experts who touch upon this question, and especially J. Thomsen Lund, who makes it the theme of his lecture, suggest the use by the underdeveloped nations of the procedures and techniques proposed by the Federal Inter-Agency River Basin Committee (*Proposed Practices for Economic Analysis of River Basin Projects* [Washington, D.C., May, 1950]). These proposals, in essence, involve a rationalization of the methods employed by United States resource-development agencies in justifying projects. The adoption of these methods by the underdeveloped countries might well serve the valuable purpose of dampening—by requiring more rigorous economic analysis—the frequently unjustified enthusiasm for giant and poorly conceived projects. It is apparent, however, that they are not suited for measuring the expansionary potential of developmental projects and programs (the "multiplier" effect and external economies created)—the very factor which is crucial in making decisions among various types of projects in countries aiming at economic expansion.

The regional study centers, and the materials which they have produced, undoubtedly signify a technical-assistance contribution of the first order. These materials, suggestive and valuable as they may be, nevertheless underline the fact that techniques of analysis and planning developed for use in the economically advanced nations generally have limited "transferability" to the less developed countries. The need today is for a theoretical structure and planning tools directly applicable to problems of economic development.

HARVEY S. PERLOFF

*University of Chicago*

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*International Economic Papers No. 1.* Translations prepared for the INTERNATIONAL ECONOMIC ASSOCIATION. London: Macmillan & Co.; New York: Macmillan Co., 1951. Pp. 244. $3.50.

This volume merits a hearty welcome as the first in a projected series containing translations of outstanding articles. Enumeration of the obvious virtues of the project is clearly superfluous.

Included in the largely unrelated selection constituting this volume are papers with topics in the theory of the firm, game theory, international trade, employment, distribution, and several papers less easily classified, among them a Soviet article on investment planning. Represented are familiar writers like Frisch, Schneider, Stackelberg, and Tinbergen, as well as others not so well known here.

I am impressed by two characteristics of the contents: the uniformly high level of performance which throughout exhibits great technical competence and the absence of anything which can really be called "revolutionary." The latter may perhaps be ascribed partly to the inclusion of several relatively early papers, much of whose contribution may have been made commonplace through diffusion.

Rather arbitrarily, I have chosen to confine my remaining comments to Bent Hansen's "Fiscal Policy and Wage Policy," one of the more recent and most novel contributions. This paper deals primarily with wage stabilization under full employment, a subject also well treated in this volume by Tinbergen along somewhat more conventional lines.

Hansen has the Scandinavian predilection for disaggregating somewhat from the Keynesian model, separating the economy into a factor (labor) market and a goods market. In the latter, excess supplies are taken to be prevented by the usual monetary and fiscal policies, with the tacit assumption that enough independent policy means are at our disposal to permit this being carried out while the factor market is manipulated in accord with the author's scheme. Thus we deal with a world of perpetual full employment. In the factor market Hansen divides wage rises into two categories: those deriving from an excess demand for labor and those occurring "spontaneously" (from trade-union pressure). Now a *ceteris paribus* increase in labor productivity is likely to decrease marginal costs and so create wage pressures via an excess demand for labor. Equilibrium can be restored by an excise tax sufficient in magnitude to offset the fall in costs. This would tend to give the increased output to the government, which could use the tax proceeds in negotiation with the trade-unions, offering these proceeds to the workers as income-tax rebates (possibly even as negative taxes). The unions would, in return, not press for spontaneous wage increases. An upward secular trend in productivity might then provide a continuing fund for union bribing, and so both sorts of wage increase might be averted. This, very roughly, is Hansen's scheme.

Aside from various administrative problems alluded to by the author, there is an important difficulty stemming from the widespread presence of monopolistic elements (which the author explicitly assumes to be absent from the goods market in his model). Monopolies and quasi-monopolies may, as Hansen indicates, tend to substitute spontaneous price changes for changes resulting from supply-demand disequilibrium. With expectations of continued full employment, spontaneous price increases are then not likely to be confined to the labor market. They may be contemplated in so many places that increased outputs will prove inadequate to bribe everyone into abstention from price-raising. Indeed, it is clearly impossible by such a redistributive device to bribe everyone at once. Unless there are enough competitive fields at whose expense we are prepared to bribe the monopolists, the device (in line with a remark of the author's) can serve merely to clarify the issues in any negotiation, making it clear that it is really the distribution of real income which is at stake. But, if that is all, it may hardly be worth the effort.

WILLIAM J. BAUMOL

*Princeton University*

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*The Economic Development of Poland, 1919–1950.* By J. TAYLOR. Ithaca: Cornell University Press, 1952. Pp. xiv+222. $3.75.

The volume under review purports "to avoid taking sides in dealing with a highly controversial area." In fact, it is an effort to present a most flattering account of the record of prewar Polish governments. It is based on very skimpy research: most of the material utilized is extracted from the *Concise Statistical Yearbook of Poland, 1939–41*, published in English by the Polish Government in Exile in London early in the war. Nor does the book reflect any critical work or any familiarity with Polish conditions on the part of the author.

For the strikingly lacking diligence, knowledge, and insight Mr. Taylor substitutes the fervor of his convictions. Thus illuminated, he finds that "the Polish government's policy of industrialization had been successful" before the war, although statistics appearing on the very same page (p. 91) indicate that only in the last two and a half years of the interwar period did Poland's industrial output exceed the desperately low levels of 1928, while in all other years industrial production was markedly below it, not to speak of the fact that even such small increases in industrial output as were secured in 1937–39 were exclusively due to an expansion of military procurement. Driven by the same zeal to prove the improvable, he depicts the landholding system of prewar Poland by giving a classification of holdings by size without taking the trouble to inform his reader about the distribution of land among the various classes. He would have had to report that 0.5 per cent of all holdings comprising 50 hectares or more controlled nearly 40 per cent of all agricultural land and over 90 per cent of all forests in the country.

While expressing on a number of occasions his "moral" indignation about the present Polish administration's measures of socialization and redistribution of income, Taylor appears to have no "moral" qualms about defending the prewar Polish regime's chauvinistic, anti-Ukrainian, anti-Lithuanian, and anti-Semitic policies. Indeed, anyone at all acquainted with the political, social, and economic life in Fascist Poland will find it terribly hard to trust his eyes when reading on page 35 that "Poland became the leading example in Europe of national economic planning on the basis of democratic control" and that it "has a good claim to being the earliest of the modern 'mixed economies' like the British economy after 1945."

It could be only the devastating impact of the "cold war" on all scholarly standards that explains why a distinguished university press has found it possible to lend the prestige of its imprint to a compilation of this sort.

PAUL A. BARAN

*Stanford University*

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*Methods of Wage Payment in British Industry.* By NORMAN C. HUNT. London: Sir Isaac Pitman & Sons; New York: British Book Centre, 1951. Pp. vii+160. $4.00.

This book is "designed to meet the needs of students preparing for the intermediate and final examinations" (p. v) of certain professional bodies in England. Like so many "textbooks," it seems to be more concerned to provide answers to the questions examiners ask in these professional examinations rather than understanding of the subject. This is a criticism of the examiners as much as of the textbook-writers.

With a quick bow in a first chapter of six pages to those who think of incentives other than financial, the author passes to some general comment on financial incentives in chapter ii. "It is beyond dispute that 'the human donkey requires either a carrot in front or a stick behind to goad it into activity' " (p. 8). Unfortunately, there is no recognition of the possible effect of the social climate of the factory on the effectiveness of any system of financial rewards and punishments. A reference to Professor T. N. Whitehead's *Leadership in a Free Society* would have been very much in point. "A wage system designed to evoke high motivation," said Whitehead, "is apt to increase the force of the motives already in being. If they are favourable to the policies of management, well and good; but if the case be otherwise, the more the employees are motivated the more they will resist their nominal leaders." The time-rate system is discussed in chapter iii; while simple and placing a "salutary emphasis on quality," it "suffers from the fundamental defect of the complete lack of financial incentive to increase output." So we turn to payment by results: some general discussion of the advantages and disadvantages (chap. iv); an examination of the extent of the system in British industry (chap. v); and a brief discussion of trade-union attitudes. On this last point the conclusion is this: "Official trade union policy to wage incentives has turned full cycle, from the definite hostility of yesterday to the full approval of today" (p. 45). There follow chapters on the individual piece-work system, premium-bonus systems, point-bonus systems, and group incentives. A chapter on merit rating follows: this is considered a way of building a "financial incentive to increase output" into a time-rate system. Chapter xiii deals with profit-sharing. This system, he concludes, "is not a satisfactory method of payment by results but it can be an excellent supplement to the more direct financial incentives, its principal value being its power to engender a team-spirit and a sense of loyalty to the working group" (p. 145). Again one wonders whether profit-sharing is not a symptom rather than a cause of good relations and high productivity; whether its effectiveness does not depend on the social climate of the enterprise. A final chapter, "Quo Vadis," contains this wise sentence: "However good, technically, a system of wage payment may be, if it has not the confidence and good will of all concerned it is doomed to failure" (p. 156). But will the student who reads this book read it in that mood?

V. W. BLADEN

*University of Toronto*

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*The Struggle for Transcaucasia (1917–1921)*. By FIRUZ KAZEMZADEH. New York: Philosophical Library, 1952. Pp. xv+366. $5.75.

This volume is one of a growing series of competent studies of significant phases of Russian history, superseding earlier accounts of the Revolution in Transcaucasia that were marked by superficiality or partisanship. Dr. Kazemzadeh obtained in the Hoover Library a surprising amount of sources in Russian, French, and English. While he has not utilized materials of the German occupants of Georgia in late 1918, this omission does not seem to have weakened his study.

It is an exceedingly complex story that the author tells. In 1917 the Georgians, the Armenians, and the Azerbaijani Turks, who inhabited the area, were closely integrated into the Russian Empire. None of them welcomed the fall of the czar or of the Provisional Government. The Bolshevik regime brought a multitude of disasters, including ferocious massacres of Azerbaijani by Armenians, and vice versa. Shortly thereafter, the Peace of Brest-Litovsk made it possible for the Turks to crush the fiercely nationalistic Armenians, while the Georgians were saved from the Turks by a welcome occupation by German troops sent to insure an oil supply from Baku. For a brief time a small British force held Baku, only to be driven out by the Turks. But Turks and Germans departed late in 1918, and new forces appeared in Transcaucasia. Ever new patterns were thrown up by the whirling kaleidoscope: Communists loyal to Moscow, Georgian Mensheviks with ties with Western socialists, Russian Whites under Denikin, Caucasian mountaineers, Persians, Turkish nationalists—all played their parts. Italy, the United States, and Britain all considered assuming control in the area but thought better of it when the Red Army triumphed over Denikin and Wrangel and the

Caucasus became untenable. Aided by local Communists, the Red Army took over Azerbaijan and Armenia, and Georgia was unable to withstand the Red tide.

Born of Persian and Russian parents, Kazemzadeh is admirably objective, with only occasional signs of sympathy for the Georgians. Implicit in his carefully documented account is the conclusion that the Transcaucasian peoples were neither ready nor able to enjoy independence. All three states made innumerable mistakes, from massacring minorities to imperialistic war on their neighbors. All leaned heavily on any handy protector and rarely enjoyed real independence. Armenia, in terrible danger from the Turks, had to accept Russian control, irrespective of the ideology. The Azerbaijani likewise could not hope to stand against their mighty neighbor. Only Georgia might have endured. But the circumstances were not propitious. Almost surrounded by Soviet territory, and with Turkey attacking from the rear, Georgia's fall was inevitable.

Kazemzadeh has written a solid rather than a brilliant account of a difficult period, with occasional errors in phrasing and spelling. The book's one large-scale map is lacking in detail; many of the places mentioned are not shown on it. These are minor flaws, however; the merits of the book far outweigh them.

JOHN SHELTON CURTISS

*Duke University*

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*Economic Consequences of Divided India.* By C. N. VAKIL. Bombay: Vora & Co., 1950. Pp. xvi + 555. Rupees 21.

Western social scientists will probably gain most from the first part of this book by Professor C. N. Vakil, director of the School of Economics and Sociology of the University of Bombay. In his fifty-page Introduction, Vakil provides a concise statement of his view of the chief economic consequences arising from the emergence in 1947 of two separate sovereign nations in the Indian subcontinent: the Dominion of Pakistan and the Republic of India. He describes in summary form the division of the land, resources, and population; the magnitude and significance of the refugee problem; the post-partition position of industries, transport, and trade; and the new structures of banking and public finance. He also analyzes the plans of India and Pakistan for economic development, with particular reference to the problems of fostering modern industry. The remaining 450 pages of the book present in an uninspired and, at times, hodge-podge manner the detailed materials upon which the introductory chapter rests. It is only fair to say that I know of no other single source which assembles so much data on these subjects.

Vakil's central thesis is simple. "Nature [*sic*] has made both India and Pakistan complementary in the economic field. . . . The maintenance of a certain [*sc.*, large] degree of economic co-operation between India and Pakistan will certainly result in large gains to both the economies" (p. 46). Since 1947, however, the trend has been toward antagonism and nonco-operation.

> Each country is trying to be self-sufficient and in trying to be so, has to deny its products to the other or to deny itself the products of the other. Questions of cost have been disregarded largely due to considerations of nationalism. The specialisation that had been evolved over a period of centuries has been shattered, and new economic patterns and relationships are being evolved in the two countries. Politics based on communal hatred and suspicion is systematically undoing the work of generations by defying the facts of economic geography and the laws of economic science [p. 355].

The main burden of the book is a plea for both nations to adopt the course indicated by the laws of economics and the dictates of nature, viz., co-operation. Thereby the national wealth of each will grow, fear and uncertainty will vanish, normal economic life will be resumed, foreign capital will flow in, and the "large amounts that are being spent on defence and refugees can be diverted for nation-building activities." In the years since November, 1950, when Vakil's book was published, little heed has been paid to his well-meant exhortation. Nor should this be altogether surprising, for Vakil's view is based upon a fundamental, but common, misreading of the mutual relations of India and Pakistan.

It is a well-known fact that the two areas which make up Pakistan today are less developed economically than the territory of India. Pakistan is a primary producer with very little industry. Almost all the modern factories in the South Asian subcontinent are located in India. When Vakil asks that India and Pakistan co-operate as complementary economies, he is in effect asking that Pakistan accept a role as a weak, dependent partner to India. Yet Vakil himself would be among the first to reject a similar economic role for *India* vis-à-vis the economies of Britain or the United States. He, like most other Indian economists, uses harsh terms to describe Western economists who urge India to accept its present junior complementary role in relation to the Western powers.

What bothers Vakil, of course, is the fact that the economies of both India and Pakistan are weak and that the struggle to develop will be long and hard. He knows that, abstractly speaking, both would gain from co-operation—in fact, that the whole subcontinent would make more progress if it went forward as a single, united economy. In the last analysis Vakil, like a follower of Hans Vaihinger, wants India and Pakistan to act as if partition had not occurred. "It was fondly believed," he writes, "that this relationship [of complementarity] in the economic field would continue undisturbed even after the partition."

What Vakil has apparently been unable to assimilate is the fact that the Pakistan movement—now the Pakistan State—has a force, a basis, and a rationale of its own. The leaders of that movement, now the heads of the new state, sought a territory, a polity, and an economy of their own. They were prepared, in prospect, for the people of their new nation to be poorer than they would have been as citizens of a united India. Now that their sphere of influence has actually been established on a national scale, the ruling groups in Pakistan are less willing than ever to be rated as juniors.

DANIEL THORNER

*University of Pennsylvania*

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*Labour Productivity of the Cotton Textile Industry in Five Latin-American Countries.* By the ECONOMIC COMMISSION FOR LATIN AMERICA. New York: United Nations Department of Economic Affairs, 1951. Pp. x+293. $3.00 (paper).

The rise of textile manufacturers in Latin America antedates by a century or more the developmental programs recently adopted by the major countries in this area to hasten their industrialization. The present status of an established industry should shed some light on the problems and prospects for new industrial adventures. In general, the record presented in this study of cotton manufacturing in Brazil, Chile, Ecuador, Mexico, and Peru—countries having 85 per cent of Latin America's productive capacity—does not offer much support for rosy prophecies.

The principal faults are found in men, machines, and managers. At the request of the Economic Commission for Latin America, a prominent firm of textile engineers devised norms, or standard productivities, for comparison with data obtained from a sample of 786 mills, varying in size, location, type of products, and age of machinery in use. In the segment of the industry (about 90 per cent of total capacity) using obsolete machinery "labour consumption per kilogramme of fabric . . . is five times greater . . . than that which could be expected under the best conditions . . . of modernity of equipment, size, organization and administration." In the "modern" segment, the mills require "82 per cent more labour (man-hours per kilogramme) . . . than would be consumed in an optimum size, well-organized and well-administered mill in which the same type of equipment is installed."

Dividing the responsibility for the industry's backwardness between "lack of extension of technical progress in matters of equipment" and "lack of extension of administrative progress," the investigation reveals that on both counts the incentives to improve have long been weak. Sheltered markets, abundant labor, and social pressure against innovation have favored antiquated patterns of work loads, cost accounting, maintenance, and organization. Excepting the few instances (conspicuously, in Chile) in which modern "know-how" has been imported, along with up-to-date machinery, the outstanding weakness of the cotton textile business is "administrative backwardness"—one of the human factors in a "sick" industry for which the remedy is neither speedy nor sure.

Close to 70 per cent of this volume is used up by elaborate tables and charts, interesting mainly to the textile engineer or mill owner. Happily, the rest of the book furnishes a sound economic analysis, country by country, of the dilemma of low productivity in an industry which, by rational standards, ought to provide a larger share of the gross national product.

ROBERT S. SMITH

*Duke University*

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*The People of Louisiana.* By T. LYNN SMITH and HOMER L. HITT. Baton Rouge: Louisiana

State University Press, 1952. Pp. xviii+272. $5.00.

Professors Smith and Hitt have applied the techniques of scientific inquiry to a tremendous mass of demographic data and have presented in this book an excellent analysis of the people of Louisiana. Their broad experience and their keen appreciation and understanding of theory are reflected by the careful selection, analysis, and presentation of data covering the people of Louisiana for a century and a half. Their analysis of the data for the last three decades, however, is much more rigorous. Their style is interesting; their graphic methods of presentation are well chosen and abundantly used; and the book is easy to read.

The analysis is based on a series of studies begun in Louisiana in 1932 and continued to the present time. Initial consideration is given to the number and spatial distribution of the people of Louisiana. Next, the results of a detailed analysis of the compositional characteristics of the people are presented. The latter includes residence, race and nativity, age, sex, marital condition, educational status, occupation, and religious affiliations. Following this, the vital processes of fertility and mortality as they prevail in Louisiana are treated in considerable detail. Then come a description and an analysis of the patterns of migration exhibited in the state. Also, the manner in which the people of Louisiana have grown in number and redistributed themselves within the state is carefully worked out and presented. Finally, the value of demographic information and principles regarding the people of Louisiana receives added emphasis in the last chapter.

Smith and Hitt have increased the interest and usefulness of their book by presenting, by way of comparison, data for other states and the nation for many of the important items. Throughout the book, pertinent relationships are established and explained, comparisons are made, trends are discussed, and policy implications are hinted at mildly, though never rigorously discussed. The usefulness of the data to individuals, firms, and the general public, however, is obvious to the intelligent reader, and, perhaps, a hint here and there as to its value is sufficient.

I realize that studies of this general nature must stick closely to the facts and relationships as they are unveiled by the analyses; however, data of the kind presented in this book always excite one's interest in the real causes which lie back of the data one observes. On this point there is need for additional infomation if one is to satisfy his curiosity as to why conditions are as the data indicate. For example, on page 144, the low birth rate among Negroes in the cotton delta area of Louisiana is given, but no explanation is offered. To chase down side alleys for a little additional information, no doubt, would interfere with the pursuit of the major objectives of the study and befuddle the presentation. Nevertheless, it is not always enough for one to know that specific conditions exist. If one is to make rational decisions, he sometimes needs to understand the causal relationships involved.

To the economist, the book is an excellent source of material. It provides a superb "jumping-off" point for analytical research into the relationships between population characteristics and economic development.

I must compliment Smith and Hitt for their excellent analysis and highly satisfactory style of presentation.

H. B. James

*Raleigh, North Carolina*

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*The Cooperative Movement and Some of Its Problems.* By Paul Hubert Casselman. New York: Philosophical Library, 1952. Pp. vii+178. $3.00.

Professor Casselman of the University of Ottawa is convinced that the consumers' co-operative movement may best serve the community by becoming a competitive yardstick for private enterprise. In his interesting little book he quite fairly states the divergent ideals of co-operative efforts in various nations and gives special attention to the relationship of co-operatives to the state. He expresses little sympathy for those who, like Dr. J. P. Warbasse, project a co-operative commonwealth which will supplant all alternative systems. Similarly, he cautions against those co-operative leaders who wish to wed co-operation to democratic socialism. Indeed, he concludes that "the greatest obstacle of the Rochdale cooperative movement during its second century of existence might very well be socialist political regimes."

Chapters are also devoted to co-operative education, to the relationship between co-operatives and the labor movement, to producer-consumer co-operative relations, and to the problem of taxation of co-operatives. While the author shows a wide acquaintance with the literature in the field, his discussion of many of these subjects is so brief that it provides barely more than a topical outline of these basic co-operative problems. One may agree with his genuine enthusiasm for co-operation as an important social reform movement and may applaud his well-formulated listings of the factors essential to the success of the effort. Nevertheless, the impression is left that he has not produced a sufficiently penetrating work to prove of great value to students of comparative economic systems. The greatest usefulness of this book will doubtless be in affording guidance to co-operative educational leaders.

COLSTON E. WARNE

*Amherst College*

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*Business in the Gilded Age: The Conservatives' Balance Sheet.* By EDWARD C. KIRKLAND. Madison: University of Wisconsin Press, 1952. Pp. iii+59. $1.50.

This slender volume presents to the general public the Kemper K. Knapp Lectures of 1950–51 at the University of Wisconsin. Witty, marked by many a bon mot, distinguished by sympathy with the conservative viewpoint, these lectures make interesting reading. The author develops his subject by analyzing the thought of Charles Francis Adams, Jr., Edwin L. Godkin, and Andrew Carnegie upon natural laws, morality, business, and the businessman. Much of the text presents a summary, with quotations, of the views of these men. Adams appears as a frustrated, defeated bureaucrat, highly critical of business because of its violation of economic and moral laws. His embarkation upon a business career in railroading after a decade of distinguished service on the Massachusetts Railroad Commission is presented as a surrender and personal tragedy. Godkin is the "prophet-publicist," applier to American business of Victorian morals and the axioms of classical economics. Like Adams, he eventually becomes completely disillusioned with the American business community. Carnegie appears as the emancipated, transplanted, self-employed handicraftsman of Dunfermline whose semi-philosophical verbalizations and glorification of the businessman as hero held the mirror up to himself as symbol of his generation of enterprisers. Necessarily short and popular, these lectures lack the qualifications that more extended treatment would introduce. Compared with the studies of the conservative thought of the era by Gabriel, Hofstadter, Twiss, Swisher, and even McCloskey, the treatment is superficial. Russ's analysis of Godkin's attitude toward the farmer is ignored, as is that publicist's critique of the "Trusts" and his advocacy of national forests. Adams' business experience in Kansas City, which paralleled his career as a railroad commissioner, is similarly at a discount. Surely so keen a scholar as Kirkland should have asked if Carnegie's naïve public attitude toward the "Trusts" was not a pose? More detailed development of the subject is needed before such an oblique attack upon the "Robber Baron" stereotype of "Gilded Age" entrepreneurship can be accepted as having validity.

CHESTER MCARTHUR DESTLER

*Connecticut College*

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*The Browns of Providence Plantations: Colonial Years.* By JAMES B. HEDGES. Cambridge: Harvard University Press, 1952. Pp. xviii+379. $6.00.

The story of the numerous and varied activities of the four Brown brothers is "the business history of the Revolutionary Era in microcosm" (p. 285). This is a superb case study. Like many case studies, it is full of detail, sometimes tedious but more often rewarding. The careful reader will obtain a good understanding of how people lived, why merchants acted as they did, and what the economic problems of the times were. Professor Hedges has written his history from the Brown papers for the years 1723–1913, meticulously preserved by Browns and now on deposit in the John Carter Brown Library and in the Rhode Island Historical Society Library. The fact that the family kept these papers over a period of six generations tells much in itself about them. It is one of the most extensive collections of business papers in this country. Within it are detailed historical records of slaving operations, an eighteenth-century spermaceti candle trust, the founding of a pre-Revolutionary pig-iron furnace, and the speculation of a mercantile family in depreciated Revolutionary War securities, to mention some of the best items. My own favorite chapter was the seventh, "Settled Correspondents in the Neighboring Governments." It sums up, better than any account I know of, the confusing operations of selling goods, ordering others, drawing bills of exchange, and keeping the business solvent.

The Brown records begin with a ledger of a small shop in Providence in 1723. Newport was the colony's leading mercantile center until the late 1760's. Obadiah and James, brothers, were small merchants. James died young, and Obadiah raised and trained his brother's four sons: Nicholas, Joseph, John, and Moses. It is with them that we are concerned. They were dissimilar but complemented one another well. Nicholas was cautious and conservative; Joseph was mechanical, academic, and sometimes indecisive; John was the extrovert, the optimist, the plunger; and Moses was the balance wheel, peaceful and humanitarian, the one who managed to compromise difficulties when they arose. Hedges portrays their characters well. During only part of this period were all four in partnership, but their business relations were generally close.

During the eighteenth century a small shop in Providence became a larger one, helped on by trading activities to the West Indies. In time the Browns became more involved in supplying other stores in Rhode Island and Connecticut. To buy the manufactured goods from England, they somehow had to get funds. This put them at first into the candle-making business—the men of near-by Nantucket had spermaceti to sell. The Browns manufactured and sold the best grades of candles. Later they went into the iron business for the same reason. Less successfully they also tried other sources of income, such as slaving. Illegal trade to the French and Dutch West Indies and trading with the enemy during the French and Indian War were likewise part of the pattern. The tiny West Indian markets were often glutted. Losses were frequent. To survive, the Browns had to be careful and yet take chances; to break laws with some, but not too much, impunity; to change and improvise and yet not give up easily; to find funds continually, although funds were scarce. Much trade literally had to be barter. One develops a profound respect for the Browns for surviving the many vicissitudes of fortune to which they were exposed.

A number of Revolutionary War episodes come to life when viewed in terms of their effects on the Browns. Among them are the nonimportation agreements, the Gaspee affair, the desperate need for saltpeter early in the Revolution, the devastating effect of the Revolution on Nantucket, the difficulties of trading with France during the war and the relief with which merchants turned to England again, the huge debt the Browns accumulated in England in the 1780's and their difficulties in paying it off, their speculation in war securities in the 1780's, and the reasons for Rhode Island's slow ratification of the Constitution.

This is a real contribution to American economic history. There is no vital single fact that stands out; it is the homely details, the personal approach, the atmosphere created that make it a classic in economic and business history literature. Readers of this book will await the second and third volumes in the Brown saga with impatience.

DONALD L. KEMMERER

*University of Illinois*

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*Men in Business: Essays in the History of Entrepreneurship.* Edited by WILLIAM MILLER. Cambridge: Harvard University Press, 1952. Pp. lx+350. $5.00.

The scholar or the man of business will find reading this book a rewarding experience. The place of this volume in the program of the Harvard Research Center in Entrepreneurial History is well stated in the Foreword by Arthur H. Cole as the third phase of a program outlined thus:

> The blocking out of an area by a few, often mature scholars; the drafting of tentative hypotheses—as in our *Change and the Entrepreneur;* and then the search for younger men who will improve on these hypotheses, who will supply empirical data that will substantiate the earlier or the improved theses, and who will continue over the years to advance research and teaching in the general subject [p. v].

*Men in Business* is the second general publication of the center. *Change and the Entrepreneur* (1949) presented some of the results in the "blocking out of an area" and the "drafting of tentative hypotheses." The current volume presents to the general public eleven essays whose goal may be said to be "a better understanding of entrepreneurial recruitment, motivation, and behavior; of the entrepreneurial role itself; and of its relation to economic development in different situations."

As the editor, William Miller, states in his excellent Introduction (p. 8), "there are no panegyrics here" (p. 1), but rather critical appraisals. Several themes recur in the essays. Thus the influence of the social and economic structure upon variation in entrepreneurship is examined in essays on "the extent to which the motivation and behavior associated with any particular role are functions of the total social structure" (p. 9) as reflected in national differences in entrepreneurial activity; on the entrepreneurial process of decision-making "as dependent upon the time when each arrived upon the scene, and upon whether he was located in a local, regional, or national community setting" (p. 115); and on the process under "bureaucratic business administration" in both mid-nineteenth century and the twentieth century. The private entrepreneur, wooing an intimate relationship with government, appears in studies of Egyptian court finance in mid-nineteenth century, of transportation developments in early nineteenth-century New York and New Jersey, and of the transformation of life insurance policies and marketing methods, 1865–1905. The relations of personal traits of character and of early environment to entrepreneurial success or failure are significantly discussed in a contrast between two international financiers of "radically different upbringing and temperament"; in a study of the effect of an early non-business and highly religious eastern environment upon the capacity of Henry Noble Day for business survival in early Ohio; and in an examination of the difficulties as a successful innovator of a person who was chiefly an engineer, Frank Julian Sprague, perfector of urban rapid transit. The influence of social stratification upon the recruitment of business leaders is exemplified particularly in the essay on the social and economic origins of leaders of American industry in the 1870's.

The most general papers are those most concerned with the methodology and significance of entrepreneurial research, *The Entrepreneur and the Social Order: France and the United States* by John E. Sawyer and *The Entrepreneur and the Community* by Robert K. Lamb. The generalizations offered are not dogmatically stated, and the tentative nature of our present knowledge of entrepreneurship is stressed again and again. Nevertheless, a sound and useful emphasis is placed upon the personalization of decision-making and upon the value of appraising these decisions in their social context. Readers may not be equally attracted by all essays, but this is inherent in the goal of the volume. In conclusion, the well-documented notes on the essays contain many useful suggestions for further reading.

JOSEPH A. BATCHELOR

*Indiana University*

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*Federal Grants and the Business Cycle.* By JAMES A. MAXWELL. New York: National Bureau of Economic Research, 1952. Pp. xiii+122. $2.00.

This little book brings together and up to date a great deal of factual material on the grant structure in this country with particular emphasis on the responsiveness of grants to business fluctuations. The data presented in 37 tables and 11 charts largely speak for themselves. The author has wisely limited his explanatory comments to a minimum. Grants have shown some countercyclical movements, but these are explained by the more general trends in government financing. There has been little utilization of the grant-in-aid as a direct instrument of countercyclical fiscal policy.

I share with Professor Maxwell the fear that any attempt to include countercyclical arrangements in the grant structure would run afoul of our political processes. The relative ease of securing increases in periods of depressed business activity would probably be more than matched by the difficulty of securing reductions during more prosperous times. The events of the past few years should have clearly demonstrated the problem of avoiding one-way flexibility in any countercyclical fiscal program. Such flexibility in a grant program would mean continued federal encroachment on state fiscal independence.

It seems unfortunate that Maxwell has seen fit to include a discussion of his favorite grant-in-aid scheme in this otherwise excellent work. This scheme, which is more fully explored in his earlier works, requires a state to show a certain standard effort before qualifying as a recipient for a grant. This effort is to be measured in terms of the percentage of state income payments devoted to the performance of the function in question. This plan, in my opinion, tends to enforce an undue uniformity on states with divergent fiscal needs. A more serious objection is that such a plan places the recipient state in a different position from other more prosperous states. It tends to penalize states because they are poor and for no other reason. Or, more correctly stated, it subjects the people in a low-income state to discriminatory federal conditions merely because relatively more poor people reside in that state. It implies, as does the whole idea of conditional grants, that the central government is somehow equipped with a knowledge of a state's own fiscal needs superior to that possessed by the state government itself.

The fiscal goal of a grant program, that of transferring income to the "poor" states because the people in those states are poor, must be kept separate from the alternative goals such as central government encouragement or preservation of certain standards of provision in the various public services. The centralist and the decentralist may unite in support of a grant program aimed at fiscal equalization; they part company when grants become instruments of federal control.

J. M. BUCHANAN

*Florida State University*

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*Conference on Business Cycles.* Held under the auspices of UNIVERSITIES-NATIONAL BUREAU COMMITTEE FOR ECONOMIC RESEARCH. New York: National Bureau of Economic Research, 1951. Pp. xii + 433. $6.00.

This volume contains four empirical papers (by Christ, Gordon, Klein, and Wright), brief summaries of two others published elsewhere (by Hultgren and Abramowitz), and several discussions of a theoretical or methodological nature (by Burns, Tinbergen, Schumpeter, Haberler, and Smithies).

No single type of analysis dominates the book, nor do the writers end up with many conclusions in common. At best the authors seem to agree in dismissing from serious consideration theories of the cycle in which monetary factors are given primary importance.

If there is any single thread which connects a substantial number of the contributions, it is probably the controversy between "model-builders" and "eclectics." The argument begins when Marschak takes issue with Burns's appraisal of the work of Wesley Mitchell. Before the book has ended, Friedman, Klein, Tinbergen, D. McCord Wright, and Koopmans have all expressed their opinions; and the empirical contributors have added a fund of tangible evidence.

On the surface the eclectics win a substantial victory, sparked by Christ's painstaking evaluation of the predictive usefulness of macroeconomic models of the Klein type. On reading that such elaborate analytical machines seem to do no better at forecasting than a "naïve model" which simply predicts persistence of current values, one looks with suspicion on empirical attempts involving the assumption of stable parameters and well-behaved random disturbances. One also wonders whether, by seeking indicators slightly less naïve than the persistence forecast, we might not substantially improve our predictive ability.

Both these suspicions are confirmed by further reading in the volume. Gordon's eclectic history of the investment boom of the twenties leaves the reader with the feeling that his understanding of what "really went on" in that decade and of what "really caused" the depression which followed has been greatly enhanced, and both Hultgren and C. Ashley Wright come forward with series that seem to promise reasonable success in the extremely difficult task of predicting cyclical turns.

Nevertheless, the victory of the eclectics is by no means complete. At least, I felt that in the contributions of Gordon, Hultgren, and Wright a disproportionately large amount of space was devoted to description of facts and regrettably little to the problems of drawing inferences from them. Gordon comes closest to presenting a validated theory—explaining the beginnings of the great depression in terms of the "overproduction" of durables of almost all types in the 1920's. But he too frequently resorts to assertions that an industry was "badly overbuilt" or that its market was approaching "saturation" in the twenties—assertions which he supports mainly by noting the declines in investment or output which subsequently took place. The more interesting questions of why the declines should be attributed to overbuilding or saturation rather than to some other cause are not entered into in sufficient detail; nor is the especially challenging problem of how the overbuilding or saturation could have been detected by an observer on the spot. Had Gordon really grappled with these questions, he would have been forced to state his theory in much more explicit and operational terms; and his contribution would have looked, in the end, much more like the product of a model-builder (though his model need not have been linear with multivariate normal disturbances).

Likewise, Hultgren's and Wright's observations of empirical regularities are really (as they both recognize) challenging starting points for economic theorizing and investigation: they are not validated hypotheses about human behavior. Economists will not (or at least should not) be satisfied until these observations can be rationalized by an acceptable theory, and that theory in turn subjected to test, processes which will inevitably entail model-building in some form or other.

Space does not permit discussion of each contribution, but the reader's attention is especially commended to Klein's study of investment behavior in the railroad and electric-light and power industries and to Smithies' remarks on the role of strategic assumptions in forecasting for policy purposes.

ARNOLD C. HARBERGER

*Johns Hopkins University*

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*History and Policies of the Home Owners' Loan Corporation.* By C. LOWELL HARRISS. New York: National Bureau of Economic Research, 1952. Pp. 224. $3.00.

In the depth of the depression, when as a result of widespread unemployment and bank failures, millions of homeowners were finding great difficulty in making mortgage payments, the Home Owners Loan Corporation was organized in an act signed on June 13, 1933, to save distressed families from losing their homes by foreclosure. On properties qualifying, the lenders holding mortgages in danger of default were offered HOLC bonds, with interest at first and later also the principal guaranteed by the federal government, for a sum usually sufficient to pay the full amount of the existing mortgage. The HOLC appraisals, based on the market price as one element, reproduction cost as a second, and valuation based on the rents of the last ten years as the third, were usually higher than the prevailing market price. A total of 1,884,356 applications for loans was received by the HOLC from June, 1933, to June, 1935, of which 92.4 per cent were received by November 13, 1934; and, of this number, 868,670 applications were rejected or withdrawn and 1,017,821 loans were closed for an aggregate amount of $3,093 million. While the HOLC was lenient in regard to payments on its mortgages, it did foreclose 194,134 properties, or 19.1 per cent of the total loans. By June, 1938, the HOLC owned 103,000 properties, but by a process of rapid liquidation this number was reduced to less than 6,000 by June 30, 1944. An average of $451 was spent on each property for reconditioning and $135 per property for maintenance. A total loss of $337 million or $1,568 per property was incurred by the HOLC. Most of its properties were sold before the recent rapid rise in home values, but it was doubtless a sound policy not to hold the properties off the market too long. In a period of very slowly rising property values, as from 1938 to 1944, the HOLC lost by not selling quickly, but the situation would have been reversed had the sales been postponed to 1946, when the rapid rise in prices would have more than offset carrying charges.

These basic outline facts concerning the operations of the Home Owners Loan Corporation and the detailed breakdown of the corporation's loan operations in each state are presented in this definitive monograph on the HOLC. The background of the legislation, the original lending activities, the characteristics of the borrowers, the loan servicing, the foreclosures on HOLC loans, the management and sale of foreclosed properties, the problems of organization and staffing, the financing of the HOLC and its financial liquidation are thoroughly discussed in successive chapters. A special sample survey was made of HOLC loans in the New York region to secure the data on the relationship between the borrower's income and family characteristics and the type of house, its cost, value, loan experience, and profit or loss on the sale. This volume is a scientific study of the lending operations of a government financial institution which can serve as a model for further studies of this type.

HOMER HOYT

*Chicago, Illinois*

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*Application of Linear Programming to the Theory of the Firm.* By ROBERT DORFMAN. Berkeley and Los Angeles: University of California Press, 1951. Pp. ix+98. $3.50.

This book probably contains the most lucid and systematic presentation of the main programming techniques now available, and the firm provides an admirable and obvious illustration. For example, it should be particularly illuminating to the student to be shown that diminishing returns are compatible with *linear* programming. Teachers and students should therefore find the book invaluable. It is regrettable, however, that many of those desiring an introduction to the subject are likely to be unequipped to follow the matrix operations in terms of which the analysis is expounded and which add greatly to its elegance. The student should also be warned that the book contains at least the usual number of typographical errors.

Programming may be described as the maximization of some function (e.g., profits), the values of whose variables are restricted in accord with relations taking the form of equations (the production function) and inequalities (scarce resources need not be used up, and inputs and outputs cannot be negative), where the explicit attention to inequalities is peculiar to the programming approach. Clearly, the mathematical programming results can be expected to be applicable to the theory of the firm.

What does this application yield? In the competitive case, where linear programming is employed, we have a technique (the Danzig "simplex method") for numerical computation which could conceivably be of some use to the firm in decision-making. Of course, our theory is far too simple, information too poor, and computation generally too slow and costly for much to be expected by way of such application for the present at least.

What else do we learn about the competitive firm? A method of imputing values to scarce factors, a duality theorem which sheds some light on the nature of that imputation but which is primarily of interest to mathematicians, and the "basic theorem," which states that a firm having *n* scarce resources generally will find it most profitable to employ *n* processes.[1] These results are all more or less dependent on the linearity assumption, whose primary justification is our inability to do much with more complicated forms. Thus in the monoply case, where the author is driven to drop the linearity assumption, rather less develops.

One is left with the impression that the results are mostly in the realm of formal manipulation and that little more is known about the operation of real firms than before. But surely it is not fair to carp at Professor Dorfman's very fine work for a characteristic which is common to so much of the theoretical literature—I am in no position to cast the first stone. Indeed, we should perhaps adopt a bolder attitude and say that such a work should be judged as a product of pure intellect, a genus which calls for no apology. It seems desirable, however, that it be clearly labeled as such, to avoid misunderstanding and to make it clear that it leaves a gap to be filled.

WILLIAM J. BAUMOL

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[1] A "process" is defined as any procedure which uses inputs and yields output in fixed proportions, any change in proportions being considered a change in process. Thus the use of fewer than *n* processes will generally not permit the firm to employ its scarce resources fully.

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